Impact of land redistribution on consumption in South Africa

An impact assessment analysis of the South African land reform

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Master thesis for the Master of Philosophy in Economics degree

Department of Economics

UNIVERSITY OF OSLO

May 2010

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http://www.duo.uio.no/

Trykk: Reprosentralen, Universitetet i Oslo

Abstract

When the Apartheid-regime fell in 1994, 87% of farmland in South Africa was owned by white farmers. The newly elected government led by President Mandela emphasized the need for a rural reform targeting the poor. The main targets in the initial phase of the reform were poverty alleviation, stimulation of economic growth and redistribution of land.

The thesis analyses the impact of the Land Redistribution for Agricultural Development-program (LRAD) in South Africa on monthly consumption expenditure per capita. The cross-section data set from the Quality of Life 2005 land reform beneficiary survey in South Africa provided by the Norwegian Institute for Urban- and Regional Research and Henrik Wiig will be used for an impact assessment analysis.

Keswell et al.(2009) provided an analysis of the average impact of the LRAD program on consumption. The authors concluded that the impact on monthly per capita consumption expenditure is positive and robust when controlling for selection bias. Following the approach used by Keswell et al. (2009), the average impact of the LRAD program on consumption is analyzed. The analysis is extended to tests of whether results are consistent in all provinces and when comparing the households of male and female household heads. Whether the LRAD program has had a positive impact on consumption expenditure per capita is the main hypothesis of the thesis.

The analysis shows a positive effect of the LRAD program on monthly consumption expenditure. The average impacts found are of lesser magnitude compared to the average treatment effects found by Keswell et al. (2009), likely due to weaker ability to reduce selection bias. In the extended analysis, tests reveal that households of male household heads on average are likely to have a positive effect on consumption from obtaining land through LRAD. The effect on female-headed households is ambiguous. Large differences are found on the provincial level. Beneficiaries in KwaZul-Natal and Gauteng exhibit large average increases in consumption, while large negative impacts are found in Eastern Cape and Mpumalanga.

Preface

The Master Thesis is written in collaboration with the Norwegian Institute for Urban and Regional Science (NIBR) as a part of project "Land at Last - Criteria for success in the South African land redistribution" jointly financed by the Norwegian Research Council and South African National Research Foundation program SOUTH AFRICA project number 180318/S50.

I would like to thank my supervisors Jo Thori Lind and Henrik Wiig, whose helpful input and feedback have been invaluable. Thanks also go to Hennig Øien for useful tips on methodology and literature, and to Øyvind Gallefoss, Åge Viken, Jacob Jorem and Michael Lee for their efforts in reviewing the thesis.

The ordinary disclaimer applies.

Jarle Dale Slørstad

Oslo, May 2010

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1 Introduction

When the apartheid-regime fell in 1994, 87% of farmland in South Africa was owned by white farmers. The first post-apartheid government in South Africa emphasized the need for rural reform targeting the poor. The initial phase of the reform had poverty alleviation, stimulation of economic growth and evening out the distribution of land as its main goals. A target was set for 30% of all white-owned agricultural land to be distributed to previously disadvantaged people within 5 years. In 2008, about five percent of the land had been transferred. The land redistribution program will be the primary focus of this thesis.

The Department of Land Affairs in South Africa commissioned a large household survey in order to review projects implemented throughout the reform. The Quality of Life-dataset (QoL) is constructed for an impact assessment-approach, where a nationally representative sample of beneficiaries can be compared to an equal sample of households in the process of obtaining land through the reform. These household can be assumed to possess the same characteristics. Using Stata 11.0, the cross-section data set from the QoL 2005 land reform beneficiary survey in South Africa provided by NIBR and Henrik Wiig will be used for an impact assessment analysis of the current reform. The data was collected in the period September 2006 to January 2007. The control group was limited to households already in the process of receiving land, to ensure that beneficiaries can be compared to households of similar characteristics.

The focus of the thesis will be to analyze the impact of the reform on consumption per capita, using the Quality of Life dataset. Keswell, Carter and Deininger (2009) analyzed the average impact of the LRAD program on consumption in the article “Poverty and Land Ownership”. The authors concluded that the impact on monthly per capita consumption expenditure is positive for the Land Redistribution for Agricultural Development-program (LRAD) households when controlling for selection bias. Though the results are robust for a variety of statistical assumptions, the magnitude of the impact is less clear cut as the size of average treatment effect varies from method to method.

Following the approach used by Keswell et al. (2009), the average impact of the LRAD program on consumption will be analyzed. The main hypothesis of the thesis is that land reform has had a positive impact on consumption expenditure per capita. In addition, the hypothesis will be tested for consistency in all provincs and will be examined with regard to gender differences among household heads.

A propensity score matching-approach will be used to attenuate the effect of factors that affect both whether a household has already obtained land and consumption expenditure, known as selection bias. The idea is to compare consumption levels for households when factors affecting treatment status are kept constant. Beneficiary households are matched and compared with households in the process of obtaining land on the basis of observable characteristics. Selection bias will be reduced if the observable factors that affect selection into the treatment group are controlled for.

A positive effect of the LRAD program on monthly consumption expenditure is found. The extended analysis reveals that households of male household heads on average are likely to have a positive effect on consumption from obtaining land through LRAD. The effect on female-headed households is ambiguous. Large differences are found on the provincial level. Beneficiaries in KwaZul-Natal and Gauteng exhibit large average increases in consumption, while large negative impacts are found in Eastern Cape and Mpumalanga.

2 Post-Apartheid South Africa

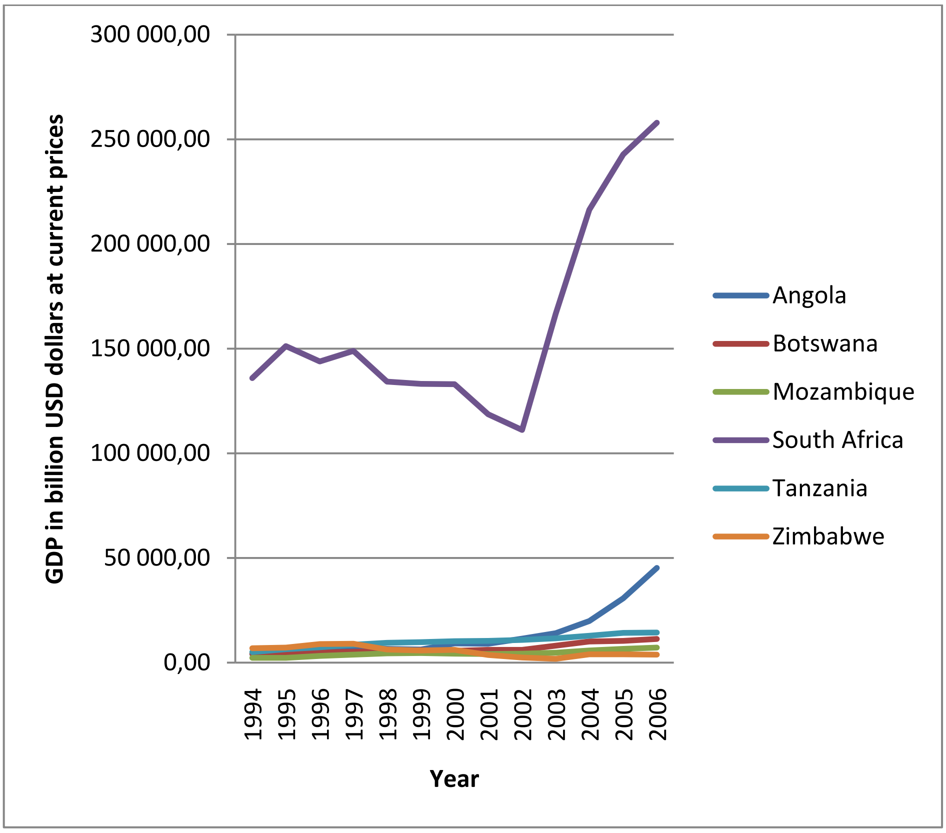
In 1994, the South African people closed the book on four decades of white apartheid rule. Although the passage from apartheid to democracy has brought immense changes, South Africa is still struggling with high unemployment and pressing inequality.

Picture 1: Map of South Africa[1]

Source: ANC (2010).

[1] Lesotho and Swaziland are not part of South Africa

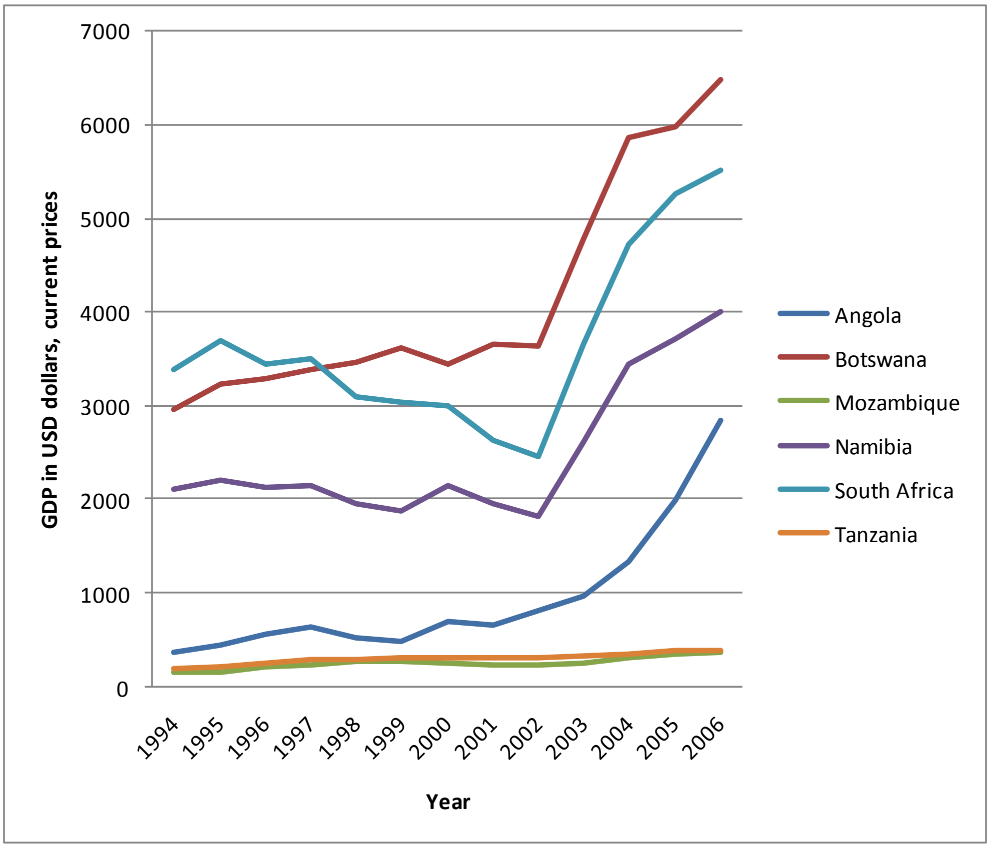
Graph 1: GDP in South Africa and its neighboring countries



Source: The International Monetary Fund (2009). GDP is given in billion USD at current prices.

South Africa is by far the largest economy in sub-Saharan Africa. The graph illustrates the difference in GDP to the neighboring countries Botswana, Zimbabwe and Mozambique, and to Tanzania and Angola, two countries in southern Africa of approximately the same size as South Africa.

Graph 2: GDP per capita in South Africa and its neighboring countries



Source: The International Monetary Fund (2009). Gross GDP per capita given in USD at current prices.

Graph 2 shows South Africa's GDP per capita compared to its surrounding countries. South Africa is second only to Botswana, after Botswana discovered and successfully invested large deposits of natural resources in the late 1990s.

Income per capita in South Africa has grown in the period from 1995 to 2005 due to expansions of social security systems, increased employment and wage growth. The positive income growth was marginally larger for the poorest than for individuals from 60th to 70th percentile. However, those on top of the income distribution benefitted even more, sustaining a skewed distribution (The National Treasury, 2008).

2.1 Poverty in the first decade of freedom

Table 1 gives percentage of the population with incomes below R174 a month, which is equivalent to an income of one US dollar a day.

Table 1: Poverty headcount rates for South Africa

	Headcount rate	
	1995	**2005**
	Poverty line: R174 a month	
African	38.18%	27.15%
Colored	14.62%	12.30%
Asian	0.82%	1.60%
White	0.23%	0.01%
Total	**30.92%**	**22.68%**

Source: (The National Treasury, 2008), p. 18.

- Table gives percentages of the population with monthly consumption expenditure (in 2000 Rands) below the poverty
- The poverty line of R174 a month is equivalent to 1 USD per day, measured in 2000 USD.

The table depicts the South African poverty as strongly dependent on ethnicity. Black Africans accounted for a disproportionate share of total poverty after Apartheid, and still does. Only 0.23 per cent of white South Africans had incomes below the poverty line, compared to 38,18 per cent of Africans by the end of Apartheid (The National Treasury, 2008). Progress made over the following decade has not improved the relative poverty situation for Africans, although the absolute income poverty was reduced from 1995 to 2005.

While only 0.01 per cent of white South Africans had incomes below the poverty line in 2005, the rate was 12.30 per cent for African descendants. The results are confirmed by the mean poverty gap levels[2] (The National Treasury, 2008).

Poverty in South Africa is a distinctively rural problem, and was therefore the initial focus of the land reform. In 2001, 46% of rural households had an income of less than USD 2 per day, while only 16% of urban households were considered as poor according to the same index (Leibbrandt, Poswell, Naidoo, Welch and Woolard, 2005).

[2] The mean poverty gap level is the mean income of individuals below the poverty line as a percentage of the poverty line.

Female-headed households still account for a disproportionate fraction of South Africa's poor. More that half of the individuals considered as poor lived in female-headed households, even though only 43 per cent of the population in total lived in such households (The National Treasury, 2008). Income poverty for women is typically associated with insufficient income. This reflects a high unemployment rate for females and low wages stemming from a relatively lower set of education. The land reform seeks to target female household heads in order to alleviate this issue (D.L.A., 2008). Thus, income poverty tends to be reproduced as a disproportionately female problem. Average increase in consumption expenditure per capita of female-headed households will in the following analysis be tested towards the equivalent result for male-headed households.

Poverty differs largely on the provincial level, both in rates of change and in absolute levels. Western Cape and Gauteng have poverty levels substantially below the national average. Gauteng, Limpopo and KwaZulu-Natal all experienced increases in both headcount rates and poverty gaps (The National Treasury, 2008). All other provinces experienced declining headcount rates of poverty.

The National Treasury (2008) concluded that the persisting inequality of wealth is largely a result of the inability of government policy to alter existing disparities in ownership, income, and the general ability of individuals to take advantage of opportunities. These entrenched inequalities reduce redistributive effects of economic growth, where already wealthy individuals possess better abilities in profiting. The program has only been able to redistribute modest amounts of land to a minority of the rural population, leaving the underlying structure of the agrarian economy in South Africa intact (Lahiff, 2008).

2.2 Demographic changes

Table 2: Population and household size

	1996	2007	1996 - 2007
Population	40.58	48.50	20%
Households	9.06	12.50	38%
Average household size	4.6	3.9	-15%

Source: (The National Treasury, 2008).

- Numbers in column 1 and 2 are given in billions.
- Last column shows percentage change from 1996 to 2007.

The population grew by 20 per cent from 1996 to 2007. At the same time, the number of households grew by 39 per cent, showing a clear unbundling of households (The National Treasury, 2008). Average households in the population exhibit different average sizes compared to the households observed in the Quality of Life-dataset. A possible explanation is that rural households are larger on average, and to a larger extent contain distant relatives and other individuals participating in the household production. (Statistics South Africa, 2008; May, Keswell, Bjåstad and van den Brink, 2009).

2.2.1 Changes the structure of the economy

Employment in agriculture has suffered from growth in other sectors. While business-services grew, the employment in agriculture, mining and manufacturing decreased substantially (The National Treasury, 2008).

Table 3: Reduction in jobs by sector, 1995-2004

	Reduction in jobs	Per cent reduction
Mining	177 000	-29.0%
Agriculture	112 000	-12.1%
Manufacturing	165 000	-11.7%

Source: (The National Treasury, 2008), p. 97.

- The last column gives reduction as a percentage of total number of jobs within the sector in 1994.

Agriculture's contribution to GDP declined substantially relative to other sectors as subsidies to the sector were reduced after apartheid. The agricultural sector has great potential in job creation and rural poverty alleviating. Increasing government support may be a necessary mean for fully utilizing this potential (The National Treasury, 2008). An important labor market trend is the growing importance of skills. People without training are not able to participate in the fastest growing sectors in the economy and seem trapped in the informal sector (Kraak, 2005).

The labor force grew at twice the rate of the growth in population and employment in the first decade after termination of the apartheid rule. The 1.6 million jobs created between 1995 and 2003 fell well short of the increase in labor supply of 4 million individuals. Thus, unemployment rose from 15 per cent in 1995 to its 2001 peak of 31 per cent. The growth of women from rural areas entering the labor force was particularly noticeable.

3 The South African Land Reform

The first post-apartheid government in South Africa emphasized the need for rural reform targeting the poor. The initial phase of the reform had poverty alleviation, stimulation of economic growth and evening out the distribution of land as its main goals. In 2008, a modest amount of approximately 5 percent of white-owned agricultural land had been transferred to previously disadvantaged individuals, a result well off the initial 30% target (Hall, 2009).

The reform consists of three dimensions: Land tenure reform, restitution and redistribution. The tenure reform was implemented to improve the land rights of individuals who had been refused to own or rent land freely, through securing tenure rights for the land where they live. Many were forcefully removed from their properties during the Apartheid rule. The restitution program was created to compensate or restore property rights for those able to prove forceful deprivation of property. Land redistribution was the main instrument to redress the gross imbalance in landholdings between whites, blacks and the colored (Deininger, 1999). In 2001, the Land Redistribution for Agricultural Development program was initiated as the main tool for redistributing land for agricultural use. This program will be the main focus of this thesis.

3.1 Initial phase

The government's initial "White Paper" on the land reform defined intended beneficiaries in broad, almost exclusively racial terms (Lahiff, 2008). The authorities emphasized the importance of maintaining public confidence in a stable land market, and decided on a willing seller, willing buyer-approach. The government was responsible for establishing a framework to accommodate transfers of land. Landowners willing to sell land could offer property to the authorities, which organized the transfer to households (Deininger, 1999).

In contrast to the land reforms in the neighboring countries Zimbabwe and Namibia, South Africa allowed for local communities to take initiative to land purchases, which the government facilitated through provision of grants (Cliffe, 2000).

The first land redistribution program was called the Settlement and Land Acquisition Grant (SLAG). It features a one-time cash grant given by the Department for Land Affairs (DLA). The program targeted mainly blacks from rural areas. In an effort to alleviate rural poverty, only those with monthly salaries below R 1500 were egliable for a grant (Deininger, 1999). The SLAG program was effective from 1995 to 2000. During that period, 1.2% of white-owned land was redistributed through the program (D.L.A., 2009).

3.2 Current reform policy

Table 5 gives province-specific information about land redistribution projects. The large differences in land areas per project can be explained by regional differences in land quality, and can partly explain why the comparatively arid Northern Cape approve larger land sizes of projects.

Table 4: Land redistribution projects initiated in 2006, by province

Province	Projects	Hectares, total	Average hectare/project	Average cost/project	Average cost/hectare
Eastern Cape	53	21 983	475	475 243	1 146
Free State	57	24 721	434	776 381	1 790
Gauteng	48	10 533	219	1 895 232	8 636
KwaZulu-Natal	54	27 808	515	2 088 804	4 056
Limpopo	15	5 574	372	674 733	1 816
Mpumalanga	48	8 808	184	1 836 765	10 009
Northern Cape	36	82 160	2 282	-	-
North West	7	2 512	359	1 744 286	4 861
Western Cape	36	135 208	3 756	3 235 358	861
Total	**354**	**319 307**	**902**	**1 412 929**	**1 566**

Source: Lahiff (2008), p. 24.

- The table shows sizes and costs of projects approved and initiated in 2006, in 2006 Rands.

3.2.1 The Land Redistribution for Agricultural Development Program

The SLAG program was succeeded by the Land Redistribution for Agricultural Development (LRAD) program. The major difference to SLAG was the removal of the income limit for applicants. Thus, the new program did not target the poorest. A minimum own contribution of R5000 was now required to secure a sufficient stake in the project. LRAD sought to a larger extent to help beneficiaries become efficient farmers, using the same market-led approach as the former program SLAG.

The main objectives of the program are outlined in the policy framework document "Land Redistribution for Agricultural Development" (The Minster of Land Affairs, 2001):

- Increase access to agricultural land for black Africans, Colored and Indians
- Overcome legacy of past racial and gender discrimination in ownership of farmland
- Facilitate structural change over the long term by assisting black people who want to establish small and medium-sized farms
- Stimulate growth in agriculture
- Expand opportunities for promising young people who stay in rural areas
- Empower beneficiaries to improve their economic and social well-being
- Enable those previously accessing agricultural land in communal areas to make better productive use of their land

The role of local authorities was promoted in LRAD, where the responsibility for dealing with applications and follow-up were delegated from national to local authorities (Cousins, 2002).

3.2.2 Gender and LRAD

LRAD targets state that no less than one third of the total amount of transferred land shall be accrued to women. As the program opens up for individual applicants, females can apply for land grants for their own right. The program guidelines stress the importance of encouraging females to apply to redress the gender imbalance in land access and ownership (The Minster of Land Affairs, 2001).

3.2.3 Sliding scale grants

Depending on the amount of own contributions, beneficiaries can access grants on a sliding scale. The minimum own contribution of R5 000 enables applicants to apply for a R20 000 grant. The maximum grants is set to R100 000, and requires an own contribution of at least R400 000. The contributions can be in the shape of cash, labor or assets. A limit of R5 000 is set for own contributions of labor, and requires a significant amount of work to be done by beneficiaries in the establishment of the project. Grants are used for acquisition of land, investments in land improvements, capital assets and infrastructure (The Minster of Land Affairs, 2001).

Grant for a specific beneficiary household is calculated on the basis of its amount of individuals of 18 years or older. Individuals can thus apply as groups to access larger grants. Group production projects are discouraged. Small-scale farmers may apply as groups, but only for the sake of group ownership with individual production.

3.2.4 Critique

A modest amount of 4.8 million hectares of the total target of transferring 24.9 million hectares of white-owned agricultural land by 2014 has been successfully transferred. A review done by the Treasury in 2008 considers the lack of post-settlement support and lack of focus on sustainable use of the land as the main reasons for the negative impact the reform has had on agricultural productive capacity (The National Treasury, 2008).

Lahiff (2008) listed several other features of LRAD and SLAG as possible reasons for the insufficient achievements. Limited evidence suggest that young people, the unemployed and farm workers have been particularly poorly served. The new targeting criteria in LRAD do not improve possibilities for these groups, as there are no national guidelines for how to prioritize and make conflicting needs for these groups meet. The reform has neither been able to increase agricultural productivity, and only to some extent been successful in redistributing land (Lahiff, 2008).

3.2.5 Current status

From 1994 to 2008, 3,123,769 housing subsidies were approved at a cost of R48.5 billion. The access to state-subsidized housing opportunities accommodated housing for almost 10 million citizens (The National Treasury, 2008). Overall, a total of R61 billion worth of housing or land assets have been transferred from the government to the South African people (D.L.A., 2009).

Since 2005, the DLA has implemented new policies, shifting towards a more supply-led approach. The responsibility of indentifying land is no longer placed entirely on beneficiaries, though grants are still available side by side with the proactive purchases.

4 Land reform theory

Productivity gains from redistribution of agricultural land are demonstrated in a large body of research. Moene (1992) states that land reforms has an unambiguously non-negative effect on production in the commercial farming sector, independent of the amount of available agricultural land.

An efficiency benefit from land redistribution arises if transfers of land from large landholders to small landholders reduce the marginal supervision costs associated with employing hired labor. Thus, transferring land from large, wage-operated farms to smaller, family-operated farms makes these costs dissipate, increasing agricultural productivity. This is known as the inverse relationship between farm size and productivity (Binswanger, Deininger and Feder, 1995).

Binswanger et al. (1995) state that economies of scale in the agricultural sector stem largely from processing and marketing, not from the farming operation itself. Further, if labor is the largest component of total costs in commercial agriculture, economies of scale in processing is not sufficient to give large farms an advantage over smaller, family-operated farms. Large farms will be more productive than smaller farms if coordination problems in processing are found in combination with economies of scale in processing. This is the only state where the inverse relationship between farm size and productivity will not be valid. Nevertheless, large landowners using hired labor will improve profitability by renting land to small-scale farmers in other states.

An apparent problem with tenancy contracts is credit rationing (Deininger, 1999). Coasean bargaining theory concludes that in the absence of impediments to efficient bargaining, competitive markets will allocate property rights to those that can use them most efficiently, irrespective of initial wealth. Efficient users will be able to compensate initial property right holders (Bardhan, Bowles and Gintis, 2001). Thus, if the landless are able to use the land more productively, overall efficiency will increase if land is transferred to them and initial landowners are compensated.

Credit constraints and imperfect information about the abilities of landless individuals will distort the market-induced efficient equilibrium, irrespective of whether a land transfer is efficiency-enhancing. This problem is known as the agency problem in incomplete markets, and may be attenuated by asset redistribution (Besley and Burgess, 2000). A land reform facilitating asset redistribution will thus enhance agricultural productivity and give lasting effects on poverty and economic growth.

During the apartheid regime, land allocations were distorted by heavy restrictions on land ownership for black individuals. To address the imbalance in land holdings, it was necessary for the South African authorities to reduce the credit constraints of the landless. The screening process in the LRAD program is an effort to mimic the competitive allocation, by only choosing individuals believed to be able to sustain a certain minimum of productivity. But as agricultural productivity is only one of the many sub-goals of the program, transfers of land are not made solely to individuals expected to increase the productivity of farms.

Pranab, Bowles and Gintis (2001) argue that mandated asset redistributions, when sustainable in the competitive equilibrium, will allow the non-wealthy to engage in productive projects that would not otherwise have been undertaken. The social welfare gains from a productivity enhancing asset redistribution accrue to the recipients. It is hard to recover the public costs of land acquisition from the beneficiaries without removing their incentives to engage in productive activities on transferred land. The society in total must bear the costs of redistribution through higher taxes. It is therefore crucial that recipients of land benefit at a level exceeding the costs (Bardhan, Bowles and Gintis, 2001).

5 Empirical background

Empirical evidence support the theoretical findings on the ability of land reforms to provide equity and efficiency benefits. Brazil and Colombia have initiated negotiated land reforms based on a willing seller-willing buyer approach.

The main goals of the land reform in Colombia in the 1960s and 70s were to correct the inequitable distribution of land and increase agricultural productivity. As in South Africa, local authorities were given large responsibilities for the implementation. The experience from Colombia showed the importance of technical support and access to credit markets, as the sustainability of initiated land reform settlements were limited. The initial approach gave little attention to improvement of agricultural productivity. Deininger (1999) highlighted two reasons for the failure of many land reform projects in Colombia. First and foremost, the absence of a fully funded plan to undertake all necessary investments was not in place. Interlinked to this issue was the lack of credit, as local authorities were unable to sufficiently reduce credit market imperfections.

Brazil has a similar approach to land reform. Results presented by Guilherme B. R. Lambais (2008) suggests that the recent Brazilian land reform has been successful in alleviating rural poverty. The effect on agricultural productivity is less clear-cut, and the failure of improvement can largely be attributed to lack of institutional assistance.

5.1 The South African Land Reform

Keswell et al. (2009) used the Quality of Life-dataset from 2005 to analyze the impact of the LRAD program on poverty alleviation in South Africa. The authors estimated the average treatment effect of obtaining land on consumption.

A combination of screening and propensity score matching was used to compare the per capita consumption expenditure of beneficiary households and control group households. The approach, by the authors referred to as a “pipeline matching strategy”, is constructed to attenuate the effect on consumption of unobservable differences in selection into the program.

Qualitative studies were made use of by Keswell et al. (2009) to map supply-side factors believed to capture determinants of selection into the treated group. The studies were used to pre-screen projects deemed unlikely to be approved, in an effort to reduce the level of heterogeneity between beneficiaries and non-beneficiaries. Passing the fourth stage of the application process was considered to be the main predictor of grant approval, and projects not meeting this criterion were screened out of the sample.

5.1.1 Empirical findings:

The various programs of the land reform were first tested for a significant treatment effect without controlling for selection bias. Impacts of restitution and SLAG programs were not significant, and tests of the Tenure Reform returned negative values. The LRAD program had a significantly postitive impact on its beneficiaries.

Keswell et al. (2009) concludes that the impact on per capita consumption expenditure is positive for LRAD households when controlling for selection bias. The robustness of the result is showed using an instrumental variable method, as well as with utilization of the alternative welfare measure consumption expenditure per adult in household. The magnitude of the average treatment effect varies from method to method. Neither can anything be said about whether these effects would be sustained, muted or reversed over time, as results apply for the short term only.

6 The Quality of Life 2005 dataset

The QoL 2005 dataset contains data on both households and the project they belong to. Of the 3751 households in the sample, 2016 are part of ongoing projects, while the remaining 1735 have statuses as applicants for a land transfer.

LRAD applications must pass five stages before land is granted (Keswell, Carter and Deininger, 2009). Approvals on the first four stages are given at a district level, before the final decision is made by the Minister of Land Affairs.

6.1 Overview of the data

Table 5: Overview of the Quality of Life dataset

Programme	Number of households	Number of individuals	Mean size of hh
Restitution - Urban	214	1421	6,6
Restitution - Rural	394	2769	7,0
Restitution - SLAG	461	3070	6,7
Restitution - LRAD	1946	12494	6,4
Redistribution - Community	106	724	6,8
Redistribution - Production/Settlement	43	254	5,9
Redistribution - Farmers Equity Scheme	44	313	7,1
Tenure - ESTA	143	909	6,4
Tenure - LTA	355	2383	6,7
Total (excluding missing values)	3706	24337	6,6
Missing values	54	312	5,8
Total	3760	24649	

Source: QoL 2005.

- The first column shows the distribution of households between programs, followed by number of individuals.

The dataset contains data collected from all land reform programs. 24649 individuals constituting a total of 3760 households were interviewed. Restitution claims have to a large extent been settled through money transfers, and will thus not be interesting in connection to my hypothesis. The focus is therefore on redistribution of land, where impact of land and land grants is more clear-cut.

Table 6: Overview of the redistribution program sample

Program	Treated	Untreated	Total
SLAG	393	67	460
LRAD	652	1294	1946
Total	**1 045**	**1 361**	**2406**

- SLAG was the initial program for accommodating transfers of rural land, and was succeeded by LRAD in 2001.
- “Treated” gives the number of households that had already received land and “Untreated” are households that are in the process of obtaining land.

The distributions of beneficiary households relative to control households in the two main redistribution programs are given in the table above. As SLAG was replaced by LRAD in 2001, it was difficult to find households still in the process of obtaining land through SLAG to be used as control households (May, Keswell, Bjåstad and van den Brink, 2009). Average treatment effects of the SLAG program will not be considered due to the lack of a sufficient base of comparison for SLAG beneficiaries.

6.2 Construction of the control group

The control group consists of land reform applicants still waiting to see their applications pass the final threshold. The sample was chosen from the population of households that had submitted their applications, but not yet obtained land grants (May, Keswell, Bjåstad and van den Brink, 2009). It can be assumed that these households possess fairly the same abilities and have the same interest in obtaining land as the households in the beneficiary group.

6.3 Problems with the data

The community data suffers from a fairly high incidence of missing data. This information should have been gathered from farm managers, reform officials and others expected to possess full records of the relevant data on each project. Missing data therefore limits the range of project-specific variables than can be utilized in the analysis.

7 Methodology

The analysis is conducted ex-post. A complete assessment of the impact on consumption expenditure per capita is not possible as only retrospective information is available about the surveyed households. An estimate of the impact can be found by comparing treated households to similar untreated households.

Statistically identifying the true impact of receiving land is a major challenge. Selection into the LRAD program is not random, and whether observations are treated or not is likely to be correlated with the dependent variable. The problem is referred to as selection bias in the literature (Ravillion, 2006; Fafchamps, 2007). Observing a state of the world where a household has obtained land and a simultaneous state where the same household has not received land is a physical impossibility. As a result, if some systematic features of the participants or the program itself take part in determining treatment status, estimates will be biased (Ravillion, 2006).

Several variables that affect selection into the program are also expected to affect consumption. If the beneficiary group and the control group have different distributions of these variables, controlling for them will not be sufficient to attenuate bias in estimates of average increase in consumption. When assignment of treatment is assumingly done on basis of observable features, it is necessary to condition on all variables that are believed to affect both income and treatment status. Problems will arise if the vector in question is large or some necessary features of the selection process are not observed (Fafchamps, 2007).

The QoL survey is constructed using a quasi-experimental design. The design is expected to be less exposed to selection bias compared to non-experimental designs, where non-participants are used as counterfactuals. The control group consists solely of individuals in the process of obtaining land. A propensity score matching approach will be used to attenuate selection bias. Beneficiary households will be matched with control households on the basis of observable characteristics. Selection bias will be reduced if the observable non-random components in the variation of selection into the program are controlled for. If non-unobservables factors affect treatment status, proxies for these effects must be used.

The average treatment effect (ATE) is the average increase in consumption attributed to the transfer of land ownership, and is given by the difference in expected consumption expenditure between treated and untreated households:

(1) $\mathrm{ATE} = \mathrm{E}(y_{i,1}|T = 1) - \mathrm{E}(y_{i,0}|T = 0)$

Adding +/- $\mathrm{E}(y_{i,0}|T = 1)$ to equation (1):

(2) $\mathrm{ATE} = \mathrm{E}(y_{i,1}|T = 1) - \mathrm{E}(y_{i,0}|T = 1) - [\mathrm{E}(y_{i,0}|T = 0) - \mathrm{E}(y_{i,0}|T = 1)]$

Equation (1) gives the single difference estimate of the treatment effect. This estimate is accurate if land transfers are randomly assigned.

By manipulating the single difference estimate, equation (2) is obtained. The first two terms constitute the average effect of treatment on those that received it. The last term picks up systematic differences between treatment and control households (Ravillion, 2006), and is likely to be non-zero in the LRAD sample due to the requirements households must meet before land is granted. Inability to isolate the treatment effect from the selection bias is known as the identification problem (Fafchamps, 2007).

7.1 Choice of dependent variable

Keswell et al. (2009) uses monthly consumption expenditure per capita in 2005 Rands as the dependent variable. Several welfare metrics can be used when considering the impact of a land transfer on living standard. Income and consumption are the most commonly used measures of welfare, as both give a fair representation of household welfare over time.

Several problems may arise when measuring welfare. Households consume some goods privately while others are consumed in part by the surrounding community. It is thus important with a consistent definition of households when collecting data to avoid extensive variation in household sizes. The QoL-survey was therefore conducted with clear guidelines on which individuals to account as part of the household (May, Keswell, Bjåstad and van den Brink, 2009).

Rural households in southern Africa are typically both producers and consumers of agricultural products. The need to keep separate records is often not considered as important by households, and may entangle measurement (Deaton, 1997). Additionally, autoconsommation[3] may cause difficulties when valuing a household's expenditure and asset base.

Problems with the measurement of consumption applies with greater force when measuring income (Deaton, 1997). Income is empirically more volatile and records of inflows are often hard for households to recall. For good estimates of income to be obtained, data on transactions must be collected with great detail; a tremendous task which is not likely to be properly executed (May, Keswell, Bjåstad and van den Brink, 2009).

7.2 Estimating the impact

Exact matching on the whole set of characteristics is not practical and may cause problems with degrees of freedom. Angrist (1998) provides an intuitive example of the complexity of exact matching. In Angrist's example, selection into treatment is determined by 11 covariates. Continuous variables must be transformed into discrete form for matching to be possible. If the 11 covariates are transformed into the simplest type of discrete variables where observations are either smaller or larger than the median, the number of patterns to be matched with the control group is:

$$2^{11} = 2048$$

Hence, even with imprecise matching on each covariate, the number of combinations that require a match to an equivalent pattern in the control group will be tremendous. Matching will give biased estimates if covariates are left out of the matching process.

7.3 Propensity score matching

To facilitate matching, a scalar index of observable characteristics is used as a basis for comparisons. The scalar index, called the propensity score, expresses the probability of being in the treated group. A probability is computed for each observation in the sample.

[3]Autoconsommation is the lack of valuation of home-produced items, as these often are consumed without being valued by a market

These predicted probabilities are computed from a regression where the outcome is a binary indicator of treatment. The idea is to isolate all factors that affect whether a household is in the beneficiary group or in the control group. The propensity score is defined as the conditional probability of receiving the treatment, given $\underline{x}$ (Keswell, Carter and Deininger, 2009):

$$p(\underline{x}) = Prob(\mathrm{T} = 1|\underline{x}) = E(T|\underline{x})$$

$p(\underline{x})$ is the propensity score, T is treatment status and $\underline{x}$ is the vector of covariates explaining treatment status.

Two theoretical results must be satisfied (Rosenbaum and Rubin, 1983):

Assumption 1 - Balance: When conditioning on the propensity score, the covariates will have equal distributions for treated and untreated households, assignment of treatment is random when comparing two household with the same propensity score:

$$\underline{x} \perp T|p(\underline{x})$$

Assumption 2 - Ignorability: For the x-vector to be able to fully determine treatment status, selection into the program must be based on observable factors. If factors affecting treatment status are omitted or not observable, estimates will be biased.

If the assumptions above are satisfied, we can write:

$$E(y|T = 1, p(\underline{x})) - E(y|T = 0, p(\underline{x})) = E(y_1 - y_0|p(\underline{x}))$$

This is the average treatment effect, conditional on the vector of covariates.

The binary variable giving treatment status is regressed on a vector of covariates believed to explain program selection. Households will be matched on basis of the predicted propensity scores. Magnitudes of the predicted estimates do not affect the outcome of the matching procedure as the score is just a diagnostic tool used to capture the non-random components of the selection process. Logit estimates take values between zero and one by construction and a logit specification will therefore be used for convenience. Comparisons of consumption can be made in cases where households with different treatment statuses have approximately the same propensity score.

7.4 Testing the balancing property

Formal tests must be implemented to assure that the control group is not statistically different from the beneficiary group when the vector of chosen variables is conditioned on. The observations are split into blocks according to their predicted propensity scores and two tests are performed (Ravillion, 2006):

1. **Balance of the propensity score**

   An equality of means-test is used to test whether the mean propensity score is the same for control and beneficiary households within each block. The outcome will show whether the propensity scores are uncorrelated to treatment assignment or not within the block. The propensity scores are split into more blocks with shorter range of propensity scores until all blocks have similar score means for beneficiaries and controls[4].

2. **Balance of each explanatory variable**

   After the correct number of blocks is determined, it is necessary to test that households in each block are similar with respect to each variable, independent of treatment status. This will confirm that the x-vector does not play a role in predicting treatment status for households with approximately the same propensity score. If a particular variable is unbalanced in a certain block, the regression specification is rejected due to an unbalanced set of explanatory variables. The strict requirements for balance ensures that households in each block are similar in all aspects captured by the the propensity score regression, so that effects cannot outweigh each other.

[4] The blocks formed in step 1 are used for stratification matching, which is explained in the section 1.5.1.

7.5 Calculating the average treatment effect

There is a trade-off between bias and efficiency in the matching process (Keswell, Carter and Deininger, 2009). Non-parametric methods do not cause severe losses of information, but may give problems of dimensionality when operating with a large x-vector. Parametric methods can cope with a large amount of x-covariates, but are suitable for smaller samples.

Three different matching approaches will be conducted:

7.5.1 Stratification/Blocking on the propensity score

When the balancing property is satisfied, treated and untreated households in each block will have the same propensity scores on average. The ATE of each block is found by the difference in mean outcome between the two groups in each block. Total ATE will be the weighted sum of the ATEs from each group, weighted according to share of observations in the block (Fafchamps, 2007).

7.5.2 Nearest-neighbor matching

Households will be matched with the closest resembling households in the control group according to propensity scores. Caliper matching is nearest-neighbor matching in its most restrictive form, and only allows for beneficiaries to be matched with a single control household. Formally (Fafchamps, 2007):

$$\widehat{ATE} = \frac{1}{N_T}\sum_{i \in I_T}\left[y_{1,i} - E\left(y_{0,i} \middle| T = 1, p(\underline{x})\right)\right]$$

Rewriting:

$$\widehat{ATE} = \frac{1}{N_T}\sum_{i \in I_T}\left[y_{1,i} - \sum_{j \in I_{TC}} W(i,j) y_{0,j}\right]$$

where N_T is the number of treated households and I_T is the set of treated observations, and $W(i,j)$ is the weighting function.

7.5.3 Kernel matching

Households are matched non-parametrically using a kernel density function. Kernel regression does not necessitate assumptions about the underlying distribution to estimate a regression function. The two previous methods use only a few control observations as comparisons to each beneficiary observation. The Kernel density function takes use of all control observations, and weights them after how much their propensity score differs from the beneficiary observation's score.

Formally (Fafchamps, 2007):

$$\widehat{ATE} = \frac{1}{N_T}\sum_{i \in I_T}\left[y_{1,i} - \sum_{j \in I_C} y_{0,j}\,\frac{K\left(\frac{p_j - p_i}{a_n}\right)}{\sum_{k \in I_C} K\left(\frac{p_j - p_i}{a_n}\right)}\right]$$

K(·) is the Kernel function and a_n is the bandwidth parameter The difference to nearest neighbor matching is the new weighting function $\frac{K\left(\frac{p_j - p_i}{a_n}\right)}{\sum_{k \in I_C} K\left(\frac{p_j - p_i}{a_n}\right)}$. The weights sum to one.

The kernel regression puts a weighted function called Kernel local to the propensity score of each beneficiary observation. A weight is assigned to each estimated propensity score in the control group based on distance from the score of the beneficiary household. The value of the Kernel function is at its maximum at the data point, and is monotonically decreasing in the distance from the maximum value (Caliendo and Kopeinig, 2005). A larger bandwidth parameter allows the function to span further from the observed data point to obtain a smooth function between two points (Becker and Ichino, 2002).

The major advantage of Kernel matching is the lower variance which is achieved from the use of more information. The drawback is the possible use of bad matches (Becker and Ichino, 2002).

7.6 The region of common support

Comparisons over scores will not be possible if the propensity score perfectly predicts treatment status. Even with an imperfect prediction, some households will not be matched due to lack of an accurate match. This may occur if observations in the control group are very unlikely to be in the treated group. Hence, a diverse sample is important. The analysis will benefit from the lack of strict guidelines for targeting of beneficiaries, in addition to a temporal benefit from the fact that beneficiaries have been accepted over a period of several years. In combination with the quasi-experimental design used, this is likely to give a sufficient number of matches so that results can be generalized to a majority of the population.

The effect of treatment is identifiable only when observations are matched with untreated observations with similar attributes. Matching will only take place on the intersecting part of the range of propensity scores. This range is called the region of common support, and estimates can only be validated for households with propensity scores in this range (Ravillion, 2006).

If, say, several females have too low p-scores to be matched with a counterfactual, and several observations of men have too high scores, the estimated treatment effect will be negatively biased. Such result may occur even if the balancing property if satisfied within the area of common support, and thus calls for separate estimations in subsamples.

8 First assessment of the dataset

Table 8 displays test statistics from simple t-tests of differences in per capita consumption between treated and untreated households in the redistribution program.

Table 7: Testing differences in expenditure between beneficiaries and controls

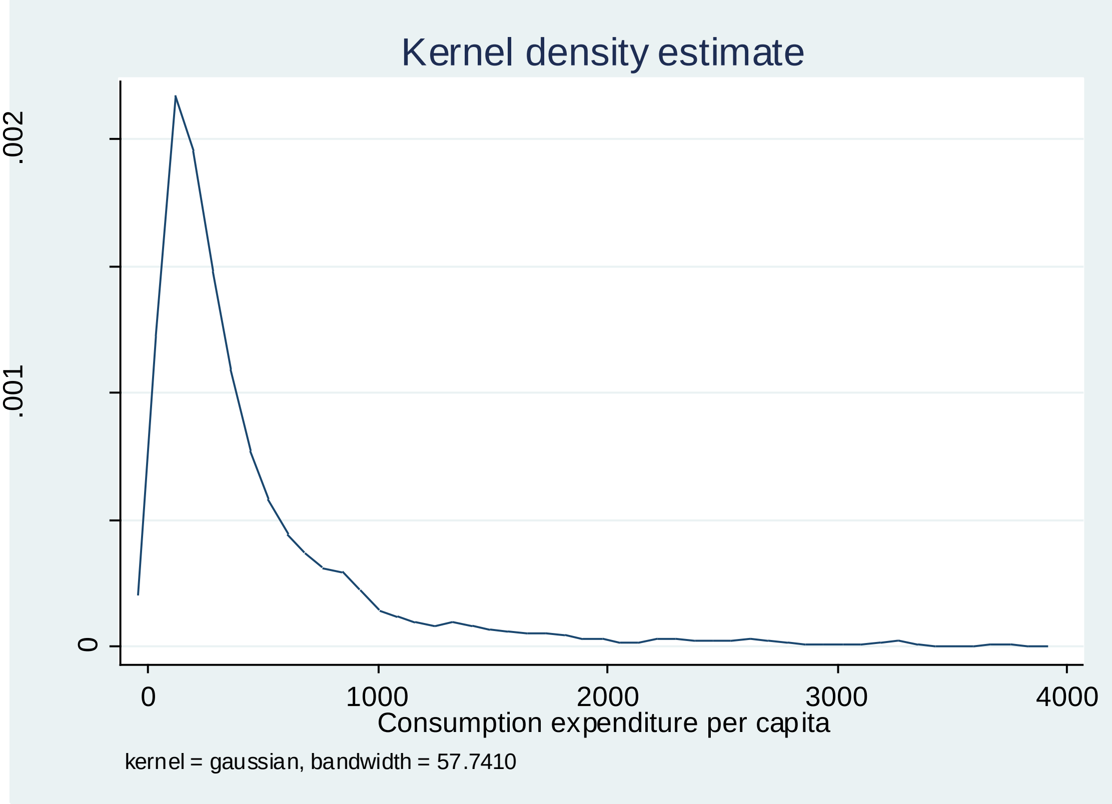
Per capita expenditure	Mean, beneficiaries	Mean, controls	Number	Prob(Diff>0)
Redistribution, total	486,67	482,30	2394	**0,4474**
	(28,44)	*(19,07)*		
SLAG	377,91	335,63	460	**0,3060**
	(33,85)	*(34,91)*		
LRAD	552,23	489,96	1895	**0,0610**
	(40,56)	*(19,96)*		

- Standard deviations are given in parentheses below each mean.
- Tests under the assumption of unequal variance did not alter conclusions.

The results of simple t-tests for differences in mean expenditure between the treated and untreated households are given in the last column. LRAD beneficiaries have significantly higher expenditure at the 10% level compared to their counterfactuals, while the SLAG program does not reveal a significant difference.

The apparent differences in mean consumption between SLAG and LRAD households reflect a shift in targeting. Removal of the maximum income ceiling and the introduction of sliding scale-grants made the LRAD redistribution program available for wealthier households compared to its precursor SLAG.

Graph 3: Per Capita Consumption Expenditure



- Graph 3 displays monthly consumption expenditure per capita, for values less than R4000, excluding 25 observations.

Graph 3 gives the distribution of monthly per capita consumption expenditure for LRAD households given as a Kernel density estimate. The distribution of consumption expenditure is severely skewed towards low-expenditure households, as indicated by the spike in the graph for low monthly expenditures.

9 Multiple regression analysis

To motivate the further analysis, monthly consumption expenditure per capita for LRAD households is first analyzed by multiple regression analysis. This framework does not control for selection bias, and will serve as a benchmark for the following propensity score analysis.

The variable “Treatment dummy” indicates whether a household is in the beneficiary group. When controlling for other determinants of consumption expenditure per capita, the value of the estimated coefficient for the dummy variable will offer insight to whether the impact of receiving land on per capita consumption expenditure is positive.

A log-specification of the dependent variable is chosen due to the positively skewed distribution of the underlying variable. The log-linear specification provides better fit. Its coefficients can be interpreted as the proximate percentage change in per capita consumption expenditure resulting from a one-unit increase in the explanatory variable (Rice, 2007).

Table 8: Regression result for log of monthly per capita expenditure consumption for LRAD households

Dependent variable: Log of consumption expenditure per capita		
	(1)	(2)
Treatment, dummy	-0.0563	-0.0029
	(-1.40)	(-0.07)
Age of household head	0.0372***	0.0381***
	(4.74)	(4.86)
Age of household head, squared	-0.0003***	-0.0003***
	(-3.75)	(-3.88)
Proportion of adults in household	0.3024**	0.3146***
	(3.23)	(3.36)
Household size	-0.2573***	-0.2563***
	(-16.67)	(-16.64)
Household size, squared	0.0078***	0.0076***
	(10.39)	(10.15)
Mean years of farming experience	0.0099	0.0102
	(1.75)	(1.81)
Mean years of education for adults	0.0232**	0.0256***
	(3.19)	(3.51)
Education of household head, years	0.0588***	0.0600***
	(12.03)	(12.26)
Gender of household head	0.2198***	0.2309***
	(5.18)	(5.43)
Dummy, interviewed in 2007		0.1348**
		(2.95)
Constant	4.7576***	4.6306***
	(20.26)	(19.44)
r2	0.3950	0.3980
N	1762.0000	1762.0000

t statistics in parentheses
* p<0.05, ** p<0.01, *** p<0.001

39.50% of the variation in log of per capita consumption is explained by model (1). All coefficients other than those of the treatment dummy and mean farming experience are significant at a 1% level. The gross effect on consumption of obtaining land is expected to be highly correlated to the other explanatory variables. As more determinants of per capita consumption are controlled for in the regression equation, the treatment dummy coefficient gets smaller. The coefficient is negative; households that have obtained land are predicted to have lower per capita consumption expenditures. However, the net effect of treatment after controlling for these variables turns out to be not significant. This can be attributed to other explanatory variables better explaining the variation in consumption.

9.1 The effect of delayed interviews

The majority of the 1939 LRAD households were interviewed in 2005. Due to several issues in the interviewing process, 36% of these households were interviewed in the beginning 2007. 91% of the households interviewed in 2007 were in the control group. If households interviewed in 2007 are structurally different from households in interviewed in 2005, consumption estimates will be biased. The effect of a late interviewing date should thus be taken into account.

To illustrate this point, a second specification of the regression equation is given in column 3 of table 9. Specification (2) includes the additional dummy variable “Interviewed in year 2007”. This variable takes the value 1 if a household was interviewed in 2007 and the corresponding coefficient gives the proximate expected percentage increase in per capita consumption that can be attributed to the delayed interview.

Households interviewed in 2007 are expected to have 12.7% higher per capita consumption than similar households interviewed at an earlier date. A comparison of the two specifications reveals that this effect is captured mainly by the treatment dummy variable when omitting the interview date-dummy. Both regressions capture approximately the same proportion the variance in the dependent variable. While the treatment dummy is predicted to have a significant effect on log per capita consumption at the 19.1% significance level in the first specification, the level has risen to 95.9% when controlling for the effect of interview delays.

It is evident that being interviewed at a later date has a certain gross effect on per capita consumption, a result further confirmed by the lack of changes in other coefficients from the inclusion of the interview dummy. As the majority of households interviewed in 2007 are in the control group, the negative bias on consumption from selection into the treated group can be attributed to a shift in beneficiary targeting.

9.2 Explaining per capita consumption expenditure

Households with young household heads are likely to have lower consumption than comparable households with an older head. The effect is decreasing in age, as the squared household age-variable has a weak, but significant negative coefficient. Members of male-headed households are expected to consume approximately 22.69% more each month than similar female-headed households. An additional year of education of the household head raises expected per capita consumption of the household by approximately 6.15%.

Living in a large household has a massive negative impact, as monthly per capita consumption expenditure is expected to be lower by 26,08% compared to a household with one less person. Economics of scale will give a weakly positive effect, given by the positive coefficient of the squared household size-variable.

The variable “Proportion of adult in the household” reflects the distribution of adults relative to children in the household. Having a 10 percentage point larger proportion of males between 15 and 60 is predicted to increase monthly consumption expenditure by 29.43%.

Education and farming experience of the household members is another main determinant of consumption. Mean values of farming experience and education affect consumption positively, with a one-year increase in the average education of the household having a predicted effect on consumption of twice the size of a similar increase in average farming experience.

The coefficient of the treatment dummy takes the value -0.0029 which tells us that a household already given land at the time of the interview is expected to consume goods worth 0.29% each month compared to control household, when holding other relevant factors constant. The coefficient is not significant at reasonable levels. Omitting the treatment dummy from the regression equation does not significantly alter magnitudes or p-values of other coefficients. Hence, the framework does not return results indicating that land transfers through the LRAD program have significantly improved per capita consumption expenditure for households.

The magnitude of the treatment dummy will be biased if features of the program or its beneficiaries systematically affect consumption. If a variable is positively correlated both selection into the program and consumption, the coefficient of the treatment dummy would be positively biased. The next section will offer an estimate of the treatment effect on consumption when controlling for selection bias.

10 The propensity score regression

Propensity score matching is constructed to attenuate selection bias. By comparing households that are equal in all aspects believed to affect selection into the LRAD program, valid estimates of treatment effect can be computed.

It is crucial to estimate an appropriate propensity score regression that captures the non-random component of the variation in selection into the program. Omitting variables will reduce the matching procedure’s ability to eliminate selection bias. The predicted propensity scores express the probability of a household being in the treated group, calculated on basis of significant differences between the two groups.

The chosen explanatory variables are tested for equality of means between beneficiaries and the control group. The results clearly indicate that assignment into the treatment group is not random.

10.1Explanatory variables

Table 9: Difference in means for propensity score variables

	Total	Treated	Control	N	diff≠0
Number of plots, pre 1995	0,649	0,663	1,312	1869	**0,000**
	1,397	*0,940*	*1,524*		
Size of plots, pre 1995	424,257	301,168	482,312	1869	**0,213**
	-2936,464	*905,336*	*3506,600*		
Land allocated post 1994, dummy	0,190	0,134	0,217	1869	**0,000**
	0,392	*0,340*	*0,412*		
Land allocated from other farmer post 1994, dummy	0,044	0,005	0,063	1869	**0,000**
	0,206	*0,071*	*0,243*		
Land allocated from tribal authorities post 1994, dummy	0,042	0,047	0,040	1869	**0,509**
	0,201	*0,211*	*0,196*		
Land allocated from municipal authorities post 1994, dummy	0,104	0,085	0,113	1869	**0,062**
	0,306	*0,279*	*0,317*		
Household head is male	0,712	0,747	0,694	1935	**0,016**
	0,453	*0,435*	*0,461*		
Education of household head, years	5,883	5,954	5,846	1932	**0,648**
	4,913	*5,005*	*4,867*		
Household size	6,632	6,426	6,736	1939	**0,117**
	4,100	*3,760*	*4,260*		
Household size, squared	60,785	55,417	63,505	1939	**0,035**
	79,916	*63,157*	*87,082*		
Age of household head	53,908	54,700	53,506	1929	**0,084**
	14,338	*13,818*	*14,583*		
Mean age of household	29,980	31,145	29,389	1934	**0,002**
	11,651	*12,875*	*10,936*		
Mean years of farming experience in household	1,609	1,793	1,516	1776	**0,112**
	3,463	*3,587*	*3,398*		
Proportion of hh members with other farming experience	0,336	0,361	0,323	1871	**0,034**
	0,365	*0,386*	*0,354*		

- Standard deviations are displayed underneath each mean value.
- The last column gives p-values from t-tests of whether mean variable value in beneficiary group is significantly different from its equivalent in control group.

Previous access to land is expected to have a negative effect on probability of being in the treated group, as the reform intends to distribute land to previously disadvantaged groups. The first two variables capture land available to households before 1995. The variable “Land allocated post 1994, dummy” indicates whether land was allocated to the household in the year 1995 or later. This variable is an aggregation of the three following dummies, which accounts for the authority responsible for the transfer.

The other variables reflect household characteristics that are expected to differ between beneficiary households and the counterparts in the control group. The dummy variable indicating whether the household head is male or not is likely to be important due to LRAD's targeting of women (Department of Land Affairs, 2009).

The LRAD program was constructed to trigger commercial farming, and the two last variables on the list reflecting farming experience of the household members will thus be important. "Mean years of farming experience in household" is the average farming experience in the household, where commercial and non-commercial agricultural experience are weighted equally. The variable "Proportion of hh members with other farming experience" gives the proportion of the adults in the household with non-commercial farming experience. The proportion is calculated on basis of the adults in the household. Adults are defined as household members between 15 and 65. Having other farming experience is a more prominent feature than having commercial farming experience among individuals in the treated group.

The remaining variables are household characteristics which can be expected to differ between the two groups due to shifts in targeting over time. Vague initial guidelines combined with the inexperience of the land reform officials have given a diversified group of treated households. Using a t-test, it is shown that household size, education of household head and age of the household head have different distribution across groups.

The variables used are believed to capture the non-random component of selection into the LRAD program. Effects that are not captured by observables must be captured through proxies. Whether structural differences can be found between the two groups when it comes to characteristics such as risk aversion, entrepreneurship and innovation will not be directly observable. The variable giving education of the household head can to some extent be expected capture such unobservable features.

Household size returns a non-significant difference in mean. Irrespective, this covariate is included in the analysis, as it is an important characteristic of a household and highly correlated to consumption per capita.

10.2 Propensity score regressions

Table 10: Propensity score regressions

Dependent variable: Treatment status

	Specification 1	Specification 2
Treatment, dummy		
Total landholdings before 1994, in hectares	-0.0000	
	(-0.80)	
Number of plots accessed before 1994	-0.6724***	-0.6081***
	(-11.18)	(-11.07)
Land allocated from other farmers after 1994	-3.1636***	-3.2243***
	(-5.29)	(-5.43)
Land allocated from tribal authorities after 1994	-0.7158**	-0.5490*
	(-2.61)	(-2.19)
Land allocated from municipal authorities after 1994	-0.6893***	-0.9605***
	(-3.56)	(-5.31)
Gender of household head	0.2400	0.2888*
	(1.87)	(2.41)
Education of household head, years	-0.0233	-0.0220
	(-1.77)	(-1.79)
Household size	0.2808***	
	(4.92)	
Household size, squared	-0.0108***	
	(-3.62)	
Age of household head	0.0045	0.0092*
	(0.98)	(2.20)
Mean age in household	0.0190**	
	(2.77)	
Mean years of farming experience	0.0170	
	(1.00)	
Proportion of adults with other farming experience	0.3665*	
	(2.09)	
Constant	-2.1684***	-0.5487
	(-4.92)	(-1.85)
Pseudo-R^2	0.1147	0.0952
N	1700	1854

t statistics in parentheses

* p<0.05, ** p<0.01, *** p<0.001

The propensity score regressions are estimated using a logit functional form. The binary treatment status variable is regressed on factors believed to determine whether households have been assigned plots, or are still in the process of obtaining land. Two specifications of the propensity score regression that will be used in the analysis are presented in table 12.

10.2.1 Matching on a large number of independent variables

Specification (1) includes a large number of independent variables. The pseudo r-squared[5] of 0.1147 is the highest of the two, and the model is thus explaining the non-random variation in treatment status best. Previous access to land has the expected negative effect. The number of plots accessed by the household during apartheid has a strong impact, while the effect of total size of plots is insignificant. Transfers of land after apartheid from the three main allocators of land do all have the expected negative coefficients.

The coefficient of the household head gender-variable is positive as expected, but is insignificant. Hence, one can conclude that the LRAD program has not been successful in its targeting of females.

The number of individuals in the household was one of the main determinants of consumption expenditure per capita in the regression equation estimated in section 9. The probability of being in the treated group is increasing size of the household. Problems of collinearity may be introduced through high correlation between covariates. However, as the following analysis is only concerned with predicted values of the propensity score and not magnitudes of the separate coefficients, this issue can be ignored.

The Department for Land Affairs proclaimed the targeting of young individuals as highly prioritized (Ministry for Agriculture and Land Affairs, 2001). As a result, the age of the household head is expected to be lower for household that have seen their applications approved. For each year older the household head is, the probability of already having received land grants increase by 0.00454. Thus, the program failed to target the young in its initial phases, although the positive coefficients may reflect a change in policy towards allowing younger individuals to pass through the first milestones in the application process. Mean age of the household returns a significant effect on treatment status. Higher mean age

[5] The pseudo r-squared ranges from 0 to 1, where 1 indicates a perfect fit. Since there is no equivalent to the R-squared calculated for ordinary least squares model for non-linear models such as logit regression models, the pseudo r-squared must be used.

increases the probability of being in the treated group. Age is likely to be correlated with other factors such as education and farming experience, which are important criteria for selection.

Experience with agricultural production is one of the main targeting criteria, as the LRAD program aims at improving South Africa's agricultural capacity. The proportion of the household with other farming experience has a significant effect on the probability of being in the beneficiary group. Average years of farming experience of a household return an insignificant coefficient at the 5% level. The variable is still included due to its theoretical importance.

10.2.2 Reducing the number of covariates

The eight covariates used in specification (2) return a pseudo-R^2 of 0.0941, which is considerably lower than for the previously described specification. The relatively weaker ability to explain treatment status can be attributed to the reduced number of explanatory variables. Conclusions about the performance of the specification the must be considered with great care. Although a lower pseudo-R^2, the set of chosen variables in specification 2 might capture the observed and unobserved characteristics of the selection more precisely. Variables that capture random variation in the selection process will not take part in removing selection bias.

As variables are excluded from the regression, coefficients and their p-values are altered. Variables giving mean age and farming experience of the households have the expected signs, though significance is impaired. The variables are nevertheless included due to their theoretical relevance. The four variables capturing households' access to land in their pre-application state have the expected negative coefficients, with land allocated from other farmers' post 1995 as the strongest predictor of treatment status.

Household gender affects probability of being in the treated group to a larger extent, compared to specification 1. Age and education of the household head have the expected coefficient values.

10.3Testing the balancing property

The predicted values of the propensity score must be tested to assure that that the group of beneficiary household is not statistically different from the control group households when conditioning on the score.

10.3.1 Balance of the propensity score

The first test is concerned with the balance of the predicted propensity score values. The estimated scores are split into intervals, and a t-test is used to confirm that the difference in mean propensity scores between the two groups is not significantly different from zero. If the difference is significant at probability values lower that 0.01, the score is split into shorter ranging intervals, and the test is performed again.

Table 11: Propensity score balance - Specification 1[6]

Block	Max p(x)	Range	N, benef.	N, control	Difference in mean	t	t, rejection
1	0,099	0,099	16	203	-0,008	-1,287	2,344
2	0,200	0,099	32	177	0,000	-0,009	2,344
3	0,300	0,099	93	274	-0,007	-2,066	2,337
4	0,400	0,100	119	251	-0,005	-1,545	2,337
5	0,598	0,198	208	233	-0,012	-2,334	2,335
6	0,762	0,160	73	21	0,000	0,003	2,368

- The estimated propensity scores are split into blocks after size of estimate.
- The largest estimated in each block is given in column 2.
- The column with the header “Difference in mean” given the difference between mean propensity score for beneficiary- and control households in the block.
- The t-values are given from a t-test for difference between score mean for beneficiaries and controls
- The critical values at a 1% level are given in the last column.
- The propensity scores will be split into smaller blocks if means differ within a block, thus if the absolute value of the t-statistic exceeds the critical value.

The values in the column “Difference in mean” are negative if the mean of the beneficiary households exceed the mean score of the control group within the block.

The estimated propensity scores from logit-specification 1 are split into six blocks satisfying the balancing property. Mean predicted propensity score differs significantly between control group and beneficiary group if the t-statistic exceeds the absolute value of the critical limit, given in the last column.

[6] The propensity scores are estimated and tested using *pscore.ado* by Becker and Ichino, presented in the article ”Estimation of average treatment effects based on propensity scores” (2002).

Table 12: Propensity score balance - Specification 2

Block	Max p-score	Range	N, benef.	N, control	Difference in mean	t	t, rejection
1	0,100	0,099	12	205	-0,003	-0,366	2,344
2	0,199	0,099	30	167	0,000	0,025	2,346
3	0,400	0,200	247	574	-0,011	-2,328	2,331
4	0,500	0,099	154	203	-0,001	-0,278	2,337
5	0,600	0,099	146	101	-0,006	-1,672	2,342
6	0,626	0,024	8	7	0,000	0,004	2,650

- The estimated propensity scores are split into blocks after size of estimate.
- The largest estimated in each block is given in column 2.
- The column with the header "Difference in mean" given the difference between mean propensity score for beneficiary- and control households in the block.
- The t-values are given from a t-test for difference between score mean for beneficiaries and controls
- The critical values at the 1% level are given in the last column.
- The propensity scores will be split into smaller blocks if means differ within a block, thus if the absolute value of the t-statistic exceeds the critical value.

The propensity score estimated on the reduced number of covariates is split into six balanced blocks. An important difference between the two specifications is how the predicted scores in the midsection of the range are split. In the latter, block three ranges from a probability of 0.199 to 0.400. A total of 821 observations are found within this block. This cluster of observations seems to be more evenly distributed towards the upper range of scores in the full specification, as the first blocks are quite similar in range and amount of observations.

10.3.2 Balance of the explanatory variables

After confirming that the blocks contain balanced propensity scores, one must ascertain that each covariate is balanced. This will confirm that the x-vector does not play a role in predicting treatment status for households with approximately the same propensity score (Ravillion, 2006). If differences in means of a variable between the two groups are significant within a block, the model specification is rejected.

Table 13: Balance of the explanatory variables - Specification 1

	1	2	3	4	5	6
Number of plots, pre 1995	0,79	0,22	0,51	0,21	0,02	-
Size of plots, pre 1995	0,76	0,68	0,62	0,68	0,85	0,59
Land all. farm., post1994	0,21	-	-	-	-	-
Land all. trib., post 1994	-	0,26	0,19	0,33	0,10	-
Land all. muni., post 1994	0,78	0,06	0,94	0,60	0,07	-
H.h. head male	0,78	0,86	0,21	0,83	0,11	0,85
Edu. h.h. head	0,27	0,09	0,41	0,28	0,74	0,96
H.h. size	0,02	0,11	0,49	0,44	0,03	0,14
H.h. size, sq	0,03	0,08	0,47	0,45	0,01	0,20
Age, h.h. head	0,69	0,55	0,63	0,59	0,28	0,66
Mean age of h.h.	0,21	0,22	0,28	0,38	0,22	0,21
Mean farm. exp.	0,76	0,49	0,58	0,78	0,94	0,35
Prop. with other farm. exp.	0,10	0,90	0,69	0,17	0,56	0,25

- Values are p-values for the mean variable value for beneficiaries not being significantly different from the mean value for controls.
- Specification is rejected if any p-values are less than 0.01.
- Values are missing when blocks do not contain both beneficiary and control observations, and when differences in means are equal to zero.

Table 14 gives p-values for the difference between mean of the variable for control households and beneficiaries being non-zero. If no means differ, the vector of explanatory variables used to explain treatment status is balanced once conditioning on the estimated propensity score. The significance level used in the analysis is 1%, so variables exhibiting values of less than this limit will be considered as unbalanced.

The occurrence of non-reported p-values is attributed to the fact that some blocks contain few observations, and tests of variables with low variation may default. This is particularly a problem for dummy variables indicating features rarely occurring in the dataset, such as land allocated by farmers.

Table 14: Balance of the explanatory variables - Specification 2

	1	2	3	4	5	6
Number of plots, pre 1995	0,39	0,33	0,61	0,81	0,23	-
Land all. farm., post1994	0,37	-	-	-	-	-
Land all. trib., post 1994	0,81	0,38	0,33	0,05	0,23	-
Land all. muni., post 1994	-	0,19	0,30	-	-	-
H.h. head male	0,93	0,16	0,25	0,38	0,49	-
Edu. h.h. head	0,72	0,02	0,76	0,56	0,50	-
Age, h.h. head	0,64	0,61	0,92	0,66	0,86	1,00

- Values are p-values for the mean variable value for beneficiaries not being significantly different from the mean value for controls.
- Specification is rejected if any p-values are less than 0.01.
- Values are missing when blocks do not contain both beneficiary and control observations, and when differences in means are equal to zero.

Table 15 exhibits the same result, variable means are statistically equal when the propensity score is kept constant. Block six contains too few observations for testing to be fully implemented, but fulfills the requirements of balance.

10.4Calculating the average treatment effect

Table 15: Average treatment effects on per capita consumption expenditure

Method	Treated obs. used	Control obs. used	ATE	SE	t
The single difference	652	1,287	**62,26***	40,25	1,54
Using propensity score regression 1:					
Stratification	541	1159	**81,09**	49,61	1,63
Nearest neighbor	541	346	**99,22**	62,28	1,59
Kernel	541	1148	**70,853***	42,86	1,65
Using propensity score regression 2:					
Stratification	597	1257	**31,65**	52,68	0,60
Nearest neighbor	652	445	**62,66**	68,70	0,91
Kernel	652	1287	**23,93**	49,67	0,48

- Values marked * are significant at the 10% level.
- The ATE-column gives average treatment effect of monthly consumption expenditure per capita on 2005 Rands.
- Standard errors from the Kernel matching are found using bootstrapping[7].

Average treatment effects on treated (ATE) are calculated using the various matching procedures explained in section 7. The column "ATE" gives the average increase in per capita

[7] When estimating the average treatment effect with the non-parametric method Kernel matching, standard errors must be estimated using bootstrapping. When the probability distribution is unknown, numerous re-samples can be randomly drawn from the sample to estimate the standard error of the distribution (Rice 2007). A large number of re-samplings are required.

consumption expenditure from attributed to receiving land. Stratification matching is based on the blocks derived in the previous section.

The single difference gives the difference between mean consumption expenditure per capita for beneficiary and control households, and is significantly positive at the 10% level. This estimate is not adjusted for selection bias.

In contrast to the non-significant treatment effect found in section 9, a positive average treatment effect significantly different from zero at the 10% level is found when using Kernel matching. Although this result is not confirmed by the other matching approaches, we can conclude that the average treatment effect of being allocated land on monthly consumption expenditure per capita is positive when controlling for selection bias.

Selection bias in program impact is present if characteristics that increase the probability of being in the treated group also affect consumption. The significant ATE-estimate from Kernel matching is larger than the single difference. Thus, estimates are higher when selection bias is controlled for, indicating a negative bias. In other words, features that affect the probability of being in the treated group are negatively correlated to consumption. The pattern is evident when comparing the two propensity score models. The average treatment effects are higher for all matching approaches when utilizing a logit specification with more independent variable and better fit. Thus, the better the model is at attenuating selection bias, the higher will estimates be.

The multiple regression analysis in section 9 provided an analysis of consumption per capita, where household size and the education level of the household head were shown to be main determinants. The same variables are used in the extended propensity score regression. Household size displayed both a negative impact on consumption a positive effect on the probability of being in the beneficiary group. An equivalent opposite correlation is found for the education level. This is an apparent source of the dominant negative selection bias. Households in the treated group are on average both larger and have less educated household heads compared to their counterfactuals. Treatment effects will be negatively biased due to the effect these variables have on household consumption. This may be due to the failure land reform officials to implement the new targeting criteria of the LRAD program.

However, the large standard deviations indicate that the differences within the sample are substantial, and suggest further testing.

Table 16: Average treatment effects found by Keswell, Carter and Deininger (2009)

Method	Definition	N, benef.	N, control	ATE	SE	t
Single Difference	Per capita			**75,18**	37,97	1,98
Estimates when using full sample:						
Stratification	Per capita	511	2154	**143,93**	56,43	2,55
Nearest neighbour	Per capita	511	303	**65,32**	73,16	0,89
Kernel	Per capita	511	1063	**134,24**	54,88	2,45
Estimates when using screened sample:						
Stratification	Per capita	349	1047	**149,87**	84,42	1,78
Nearest neighbour	Per capita	394	143	**148,28**	91,54	1,62
Kernel	Per capita	394	382	**169,18**	77,55	2,18

Source: Keswell, Carter & Deininger (2009), p. 17.

- Estimates in the last section are computed on basis of a reduced sample. The authors used qualitative surveys to gather information about households in the control group, and screened out projects deemed unlikely to be approved.

Keswell et al. (2009) estimated substantially higher average treatment effects. The authors used results from a separate qualitative survey to construct the variables “Days in the application process” and “Distance from project plot to closest Land Reform office”. These variables were used in the propensity score regression to isolate treatment status. As these variables were not available for my analysis, it is reasonable to assume that the estimates found were higher due to better ability to capture the negative selection bias.

The screened sample exhibit stronger impacts of the land reform. The qualitative surveys were conducted in an effort to ensure similarity between beneficiaries and the counterfactuals. The higher estimates are due to reduced homogeneity in the control group, indicating a stronger relationship between land transfers and consumption compared to the findings presented here.

Nevertheless, whether results are sustained in subsamples were not tested by Keswell et al. (2009).

10.5Differences in average treatment effects between male and female-headed households

The large standard deviations found in the previous analysis suggest further testing. This section will look at differences in treatment effects for households with male and female household heads by separate matching within these subsamples. Differences between subsamples may indicate inappropriateness of the propensity score method on the full sample if the region of common support is too limited.

Table 17: Gender of household head by treatment status

	Female	Male	Total
Control households	392	891	1,283
Beneficiary households	165	487	652
Total	557	1378	1,935

- Table shows the number of LRAD households with male and with and female household heads split into control- and beneficiary groups.

71.24% of the sampled LRAD households have a male head. As described in section 2, female-headed households account for a disproportionate fraction of South African households with incomes below the poverty line. Targeting of females have been one of the main priorities of the redistribution program (Department of Land Affairs, 2008). It is therefore of great interest to see whether the reform in isolation has been able to alleviate poverty of this specific group.

A specification including all suitable explanatory variables will be used to estimate propensity scores in the two subsamples. Intuitively, the dummy variable indicating the gender of the household head cannot be included in these specifications. Slight alterations to adapt to the features of the subsamples are therefore necessary for the logit regressions to satisfy the balancing properties.

Table 18: Propensity score regressions for the household head gender subsamples

Dependent var.: Treatment status

Specification	**Males, 1**	**Males, 2**	**Females, 1**	**Females, 2**
Treatment, dummy				
Total landholdings before 1994, in hectares	-0.0000		0.0002	
	(-1.01)		(1.12)	
Number of plots accessed before 1994	-0.7304***	-0.6537***	-0.5696***	-0.5151***
	(-10.20)	(-9.99)	(-4.76)	(-4.95)
Dummy, land allocated post95	-1.1348***	-1.2881***	-0.9546**	-0.9260***
	(-5.90)	(-7.18)	(-3.22)	(-3.43)
Education of household head, years	-0.0263	-0.0291*	0.0050	0.0101
	(-1.74)	(-2.07)	(0.19)	(0.40)
Household size	0.2874***		0.2579*	
	(4.27)		(2.27)	
Household size, squared	-0.0100**		-0.0130*	
	(-2.87)		(-2.06)	
Age of household head	0.0105	0.0146**	-0.0036	0.0023
	(1.84)	(2.87)	(-0.43)	(0.31)
Mean age in household	0.0149		0.0278*	
	(1.88)		(2.02)	
Mean years of farming experience	0.0123		-0.0079	
	(0.66)		(-0.21)	
Proportion of adults with other farming experience	0.6602**			
	(3.24)			
Constant	-2.2277***	-0.4447	-1.8455*	-0.4491
	(-4.39)	(-1.36)	(-2.19)	(-0.85)
Pseudo-R^2	0.1234	0.0949	0.0745	0.0545
N	1210	1320	490	534

t statistics in parentheses

* p<0.05, ** p<0.01, *** p<0.001

The variable giving proportion of household members with non-commercial farming experience is omitted in the subsample of female heads due to failure to satisfy the balancing property.

The coefficient has the expected signs in the subsample of male-headed households. Several coefficients are deviating from its expected values in the subsample containing the female-headed counterparts. Years of education affect the probability of being in the beneficiary group positively, in contrast to the effect for males. Another difference is the negative coefficient of the age variable for household heads in the female subsample. Female household heads do exhibit higher education and lower age, more in line with the targeting criteria of the reform (Ministry for Agriculture and Land Affairs, 2001).

The coefficient of mean farming experience in the household is negative for females, which is surprising with regards to the outline of the LRAD program. A possible explanation can be the targeting of women, allowing women into the program even though the household in question lacks the ideal farming experience. The coefficient also reflects that the female-headed households in the control group have more farming experience on average, indicating improved targeting of experienced beneficiaries.

The specification used to predict treatment status for male heads has a substantially better fit than the models for female household heads. This may be attributed to the substantially lower number of observations in the sample of female-headed households, and the fact that the full sample on which the initial regressions were calculated on the basis of consists of mainly male household heads. As seen in section 10.4, there is a sharp decline in model fit when the amount of explanatory variables is reduced.

The balancing property is satisfied both for all predicted propensity scores and for the corresponding vector of explanatory variables used[8].

[8] Tests of the balancing property are given in the Appendix

Table 19: Average treatment effects on per capita consumption expenditure for households with male heads

Method	Treated obs. used	Control obs. used	ATE	SE	t	t, rejection
Using propensity score regression 1:						
Stratification	403	807	**91,12**	63,39	1,44	1,65
Nearest neighbor	487	334	**154,19***	78,60	1,96	1,65
Kernel	487	891	**85,72***	62,61	1,74	1,65
Using propensity score regression 2:						
Stratification	450	870	**39,91**	64,91	0,62	1,65
Nearest neighbor	487	334	**32,89**	85,15	0,39	1,65
Kernel	487	891	**21,01**	65,23	0,32	1,65

- The ATE-column gives average treatment effect of monthly consumption expenditure per capita on 2005 Rands.
- The critical values in the column headed “t, rejection” are given at a 10% significance level.
- Values marked * are significant at the 10% level.
- Standard errors from the Kernel matching are found using bootstrapping.

Table 20: Average treatment effects on per capita consumption expenditure for households with female heads

Method	Treated obs. used	Control obs. used	ATE	SE	t	t, rejection[9]
Using propensity score regression 1:						
Stratification	132	358	**51,80**	72,45	0,72	1,65
Nearest neighbor	165	125	**-13,51**	79,78	-0,17	1,65
Kernel	165	392	**40,56**	64,32	0,63	1,65
Using propensity score regression 2:						
Stratification	147	387	**24,36**	69,53	0,35	1,65
Nearest neighbor	165	125	**58,08**	79,08	0,73	1,65
Kernel	165	392	**30,72**	65,27	0,47	1,65

- The ATE-column gives average treatment effect of monthly consumption expenditure per capita on 2005 Rands.
- The critical values in the column headed “t, rejection” are given at a 10% significance level.
- Values marked * are significant at the 10% level.
- Standard errors from the Kernel matching are found using bootstrapping.

Average impacts for male household heads are the only significant impacts when using nearest neighbor matching and Kernel matching on basis of the full propensity score regression. It is thus hard to conclude on differences in magnitudes. However, it can be stated that the households in the sample headed by males have a positive effect on consumption from receiving land through the LRAD program, while one cannot conclude whether female-headed households have had any significant effect of the reform.

The pattern of negative selection bias described in section 10.4 is also seen here, estimates are increasing in the amount of explanatory variables that are conditioned on. The higher estimates of households with a male household head can thus possibly be attributed to the better fit of the propensity score regressions used when matching male-headed households. It is also reasonable to assume that stronger effects would have been found if the propensity score models had provided better ability to reduce selection bias, in particular for female household heads. If the inability to explain treatment status for female households is due to a more randomized selection, results will not be distorted by selection bias.

10.6Testing for provincial differences in average treatment effects

Table 23 sums up the estimated average treatment effects by province. Specification 2 with the additional explanatory variable "Proportion of household members with farming experience" is used in the logit regression explaining treatment status on which the estimated propensity scores are matched. The logit models used to predict treatment statuses provide good fit in line with the models previously used, although the number of explanatory variables is reduced. The model specification used satisfies the balancing properties for all provinces[10].

Table 21: Summing up average treatment effects on per capita consumption expenditure by province[11]

	Control, N	Benef., N	Stratification	Nearest neighbor	Kernel
Limpopo	41	108	113,52*	80,25	92,11
Mpumalanga	155	242	-219,22*	-467,86*	-225,96
North West	189	187	95,63	133,78	107,84*
Gauteng	52	135	369,12**	400,09***	306,69
Northern Cape	30	200	-106,14	18,98	-178,86
KwaZulu Natal	490	495	447,85**	360,53*	433,45**
Free State	236	281	86,51	88,61	93,19
Western Cape	309	104	11,03	73,97	11,89
Eastern Cape	199	251	-615,22**	-481,47*	-593,38

* estimate significant at the 0.1-level
** estimate significant at the 0.05-level
*** estimate significant at the 0.01-level

[10] Full tables of regressions, estimations and the balance of the propensity score and the variables are given in the appendix.
[11] Detailed data on the matching is provided in table 30 in the Appendix.

The provincial differences are substantial and clear-cut. Impact on per capita consumption expenditure attributed to the transfer of land varies greatly among the provinces. Mpumalanga and Eastern Cape has a severe negative treatment effect that is significant at reasonable levels. At the other end of the scope, the estimates of average treatment effects found for the Gauteng and KwaZulu-Natal stand out with large and significant effects of LRAD projects. Hence, some provinces can be said to be very successful in their effort to promote poverty alleviation through the framework of the reform, while others have failed considerably.

As described in section 2.1, poverty differs greatly on the provincial level. The two regions with headcount poverty rates substantially lower than the national average, Gauteng and Western Cape, exhibit large differences in average treatment effects. The average performance of Gauteng beneficiaries is second only to beneficiaries in KwaZulu-Natal. Apart from Western Cape and Gauteng, South Africa's provinces are similar in their fractions of the population having consumption expenditures per capita beneath the poverty line. The two poorest provinces are Eastern Cape and Limpopo (The National Treasury, 2008). The unambiguously negative treatment effect of land transfers in the Eastern Cape Province is in stark contrast to the positive effect found in Limpopo. These differences are likely to be attributed to differences in targeting by local authorities.

Due to insufficient data provided by the Department of Land Affairs, comparisons over average costs and project sizes between provinces cannot be made. There were no national requirements for reporting of initiated projects in the period in which this thesis is concerned. This has resulted in extensive variation in the project-specific information reported on the provincial level, which puts severe restrictions on the ability to analyze differences in provincial treatment effects.

Whether differences between the provincial estimates are statistically significant is beyond the scope of this thesis. Further analysis on this matter is suggested. However, estimates are fairly precise and consistent for the variety of matching approaches, indicating a statistical difference.

11 Conclusion

The average effect on monthly consumption expenditure per capita of being allocated land through the Land Redistribution for Agricultural Development-program is positive. A negative selection bias is apparent, as average treatment estimates are positively correlated to the ability of the propensity score regression to reduce bias. The negative selection bias can be attributed to features that are both negatively correlated to consumption and positively affecting the probability of being in the treated group. Examples of such features are household size and education of the household head. The results are not robust to the variety of matching methods applied, as the Kernel matching approach is the only method returning a significantly positive average treatment effect.

The average impacts found are of lesser magnitude compared to the average treatment effects found by Keswell et al. (2009). The results are likely lower due to weaker ability to reduce selection bias.

Testing of the subsamples of household with male household heads and female household heads separately revealed that impacts of the LRAD program depends on the gender of the household head. The results indicate that households with male heads have a positive effect on consumption per capita from receiving land through the LRAD program. The results provide no clear answers to whether female-headed households realize significant effects of the reform. The estimate of average treatment effects for males was substantially higher than the estimate for the full sample, which is an indication that male-headed households realized a greater average effect from obtaining land trough LRAD. A negative selection bias is found in these subsamples and results may be distorted if the inability to capture factors affecting the probability of being in the treated group is due to unobservable systematic factors and not due to randomized selection.

Estimations of average treatment show large provincial differences. Mpumalanga and Eastern Cape showed large negative average impacts on consumption, while estimates for Gauteng and KwaZulu-Natal were large and positive. It is therefore of great importance to review the LRAD program at a provincial level.

The differences are likely to be attributed to differences in targeting by local authorities. The two richest provinces Gauteng and Western Cape exhibit large differences in average treatment effects, the same is the case for the two poorest provinces, Eastern Cape and Limpopo. As the reported data on the LRAD program in the period 2001-2006 has several severe shortcomings, it is hard to conclude on a relationship between average project sizes, average costs and average impacts on consumption.

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12 Appendix

Table 22: Propensity score balance for households with female heads

Block	Max p(x)	Range	N, benef.	N, control	Difference in mean	t	t, rejection
1	0,200	0,199	15	126	-0,031	-2,055	2,353
2	0,400	0,200	85	184	-0,019	-2,502	2,340
3	0,491	0,089	24	44	-0,006	-1,122	2,384
4	0,576	0,075	3	8	-0,013	-0,782	2,821
5	0,606	0,000	0	1	-	-	

- The estimated propensity scores are split into blocks after size of estimate.
- The largest estimated in each block is given in column 2.
- The column with the header “Difference in mean” given the difference between mean propensity score for beneficiary- and control households in the block.
- The t-values are given from a t-test for difference between score mean for beneficiaries and controls
- The critical values at the 1% level are given in the last column.
- The propensity scores will be split into smaller blocks if means differ within a block, thus if the absolute value of the t-statistic exceeds the critical value.

Table 23: Balance of the explanatory variables for households with female heads

Specification 1

	1	2	3	4
Number of plots, pre 1995	0,06	0,98	0,51	-
Size of plots, pre 1995	0,40	0,22	0,30	-
Land all. post 1994	0,02	0,06	-	-
Edu. h.h. head	0,37	0,87	0,68	0,03
H.h. size	0,03	0,54	0,67	0,61
H.h. size, sq	0,05	0,31	0,90	0,48
Age, h.h. head	0,18	0,62	0,73	0,52
Mean age of h.h.	0,93	0,85	0,52	0,71
Mean farm. exp.	0,27	0,62	0,69	0,39

- Values are p-values for mean variable value for beneficiaries not being significantly different from the mean value for controls.
- Specification is rejected if any p-values are less than 0.01.

Table 24: Propensity score balance for male household heads, specification 1

Block	Max p(x)	Range	N, benef.	N, control	Difference in mean	t	t, rejection
1	0,200	0,197	38	269	-0,017	-1,934	2,339
2	0,400	0,200	135	338	-0,006	-1,125	2,334
3	0,499	0,098	74	106	-0,001	-0,224	2,347
4	0,598	0,097	83	53	-0,011	-2,093	2,354
5	0,800	0,199	73	35	-0,019	-1,754	2,362
6	0,812	0,010	6	0	-	-	

- The estimated propensity scores are split into blocks after size of estimate.
- The largest estimated in each block is given in column 2.
- The column with the header "Difference in mean" given the difference between mean propensity score for beneficiary- and control households in the block.
- The t-values are given from a t-test for difference between score mean for beneficiaries and controls
- The critical values at the 1% level are given in the last column.
- The propensity scores will be split into smaller blocks if means differ within a block, thus if the absolute value of the t-statistic exceeds the critical value.

Table 25: Balance of the explanatory variables for male household heads

Specification 1

	1	2	3	4	5
Number of plots, pre 1995	0,16	0,10	0,38	0,27	0,65
Size of plots, pre 1995	0,45	0,48	0,97	0,0681	0,77
Land all. post 1994	0,23	0,48	0,19	0,08	-
Edu. h.h. head	0,08	0,81	0,83	0,04	0,60
H.h. size	0,06	0,30	0,03	0,36	0,77
H.h. size, sq	0,05	0,35	0,02	0,32	0,56
Age, h.h. head	0,58	0,22	0,24	0,92	0,59
Mean age of h.h.	0,11	0,62	0,23	0,21	0,15
Mean farm. exp.	0,55	0,19	0,67	0,71	0,18
Prop. with other farm. exp.	0,02	0,08	0,70	0,87	0,71

- Values are p-values for mean variable value for beneficiaries not being significantly different from the mean value for controls.
- Specification is rejected if any p-values are less than 0.01.

Table 26: Average treatment effects on p.c. consumption expenditure for households with male heads

Method	Treated obs. used	Control obs. used	ATE	SE	t
Using propensity score regression 1:					
Stratification	403	807	91.122	63.389	1.437
Nearest neighbor	487	334	154.187	78.601	1.962
Kernel	487	891	85.722	62.608	1.369
Using propensity score regression 2:					
Stratification	450	870	39.912	64.914	0.615
Nearest neighbor	487	334	32.893	85.147	0.386
Kernel	487	891	21.007	65.232	0.322

- The ATE-column gives average treatment effect of monthly consumption expenditure per capita on 2005 Rands.
- The critical values in the column headed "t, rejection" are given at a 10% significance level.
- Values marked * are significant at the 10% level.
- Standard errors from the Kernel matching are found using bootstrapping.

Table 27: Average treatment effects on p. c. consumption expenditure for households with female heads

Method	Treated obs. used	Control obs. used	ATE	SE	t
Using propensity score regression 1:					
Stratification	132	358	51.802	72.445	0.715
Nearest neighbor	165	125	-13.506	79.783	-0.169
Kernel	165	392	40.559	64.315	0.631
Using propensity score regression 2:					
Stratification	147	387	24.362	69.533	0.350
Nearest neighbor	165	125	58.078	79.080	0.734
Kernel	165	392	30.718	65.272	0.471

- The ATE-column gives average treatment effect of monthly consumption expenditure per capita on 2005 Rands.
- The critical values in the column headed "t, rejection" are given at a 10% significance level.
- Values marked * are significant at the 10% level.
- Standard errors from the Kernel matching are found using bootstrapping.

Table 28: Propensity score regressions for province samples

	Limpopo	Mpuma.	North W.	Gauteng	N. Cape	KwaZulu-N.	Free S.	W. Cape	E. Cape
Treatment, dummy									
Pr. of adults with other farming	0.9896	0.9814*	1.1013*	0.0833	0.6016	-1.0943*	0.5430	0.7798	-0.4669
	(1.04)	(2.01)	(2.21)	(0.09)	(0.49)	(-2.07)	(1.36)	(1.67)	(-1.31)
No. of plots accessed before 94	-0.2110	-0.6805***	-0.2003	3.4032**	-1.0844*	-0.4668***	-0.9417***	-0.0955	-1.3371***
	(-0.50)	(-4.02)	(-1.03)	(2.84)	(-2.57)	(-4.71)	(-4.72)	(-0.29)	(-6.01)
Land all. from farmers after 94	0.0000	-0.2625	0.0000	0.0000	0.0000	-2.5959*	0.0000	-1.5837	0.0000
	(.)	(-0.20)	(.)	(.)	(.)	(-2.48)	(.)	(-1.42)	(.)
Land all. from tribal auth. after 94	-2.3245*	0.0000	-1.5798	0.0000	0.6126	0.1394	0.0000	0.6218	0.3072
	(-2.28)	(.)	(-1.26)	(.)	(0.40)	(0.31)	(.)	(0.59)	(0.39)
Land all. from muni. auth. after 04	0.8414	0.0000	-1.6726**	0.0000	-1.5035	0.7618	-0.2778	-1.0293	-1.8128***
	(0.64)	(.)	(-2.70)	(.)	(-1.47)	(0.86)	(-0.81)	(-1.47)	(-3.93)
Gender of household head	0.0305	0.8998*	-0.3737	0.8426	-0.5043	1.0601**	0.5242	-0.1579	-0.0003
	(0.04)	(2.18)	(-1.07)	(1.07)	(-0.60)	(2.97)	(1.61)	(-0.38)	(-0.00)
Edu. of household head, years	-0.1219	-0.1059*	0.0109	0.2601*	0.0916	-0.0476	-0.0364	-0.0130	-0.0624
	(-1.66)	(-2.18)	(0.27)	(2.25)	(1.10)	(-1.22)	(-1.03)	(-0.28)	(-1.87)
Age of household head	-0.0078	-0.0190	0.0019	-0.0337	0.0124	0.0116	-0.0240*	0.0207	0.0546***
	(-0.27)	(-1.11)	(0.14)	(-1.16)	(0.42)	(1.02)	(-1.97)	(1.53)	(4.55)
Constant	jan.30	0.2624	0.0131	-3.2233	0.2700	-1.6804*	1.0122	-1.7537	-1.4188
	(0.64)	(0.25)	(0.01)	(-1.66)	(0.12)	(-1.99)	(1.13)	(-1.69)	(-1.75)
Pseudo-R^2	0.1551	0.2016	0.0718	0.2190	0.1663	0.1674	0.1053	0.0574	0.2326
N	64	186	178	74	53	418	288	172	300

t statistics in parentheses

* p<0.05, ** p<0.01, *** p<0.001

Table 29: Propensity score balance for propensity score regressions for each province

Limpopo

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,171	1	5	-0,094	-
2	0,387	2	10	0,057	1,4365
3	0,578	10	9	0,019	0,6859
4	0,798	5	20	-0,001	-0,0264
5	0,860	1	1	0,045	-
		19	45		

Mpumalanga

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,096	1	37	-0,009	-
2	0,194	9	20	0,000	-0,027
3	0,391	10	38	-0,036	-1,8069
4	0,590	18	19	-0,004	-0,2168
5	0,794	23	10	-0,035	-1,682
6		0	1		
		61	125		

North West

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,188	20	4	-.027409	-2.1549
2	0,400	14	24	-.023596	-1.4050
3	0,598	47	51	-.0230869	-2.2320
4	0,788	11	7	-.0150868	-0.6127
		92	86		

Gauteng

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,194	2	21	-0,087	-2,250
2	0,395	7	16	-0,019	-0,663
3	0,415	8	1	0,009	-
4	0,593	2	21	-0,552	1,956
5	0,702	0	3	-	-
6	0,950	8	0	-	-
		27	62		

Northern Cape

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,151	0	6	-	-
2	0,385	4	6	0,072	3,007
3	0,574	7	10	-0,016	-0,569
4	0,757	11	2	-0,088	-2,417
5	0,880	5	2	0,008	0,288
		27	26		

KwaZulu Natal

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,198	23	281	-0,021	-1,894
2	0,399	21	60	-0,018	-1,084
3	0,592	19	12	-0,032	-1,750
4	0,683	1	1	-0,075	-
		64	354		

Free State

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,198	12	73	-0,001	-0,089
2	0,299	10	35	-0,024	-2,476
3	0,398	22	34	-0,006	-0,750
4	0,599	37	49	-0,023	-1,838
5	0,760	13	3	0,004	0,162
		94	194		

Western Cape

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,199	5	27	-0,024	-1,198
2	0,398	27	70	-0,002	-0,190
3	0,537	19	22	-0,004	-0,376
4	0,659	2	0	-	-
		53	119		

Eastern Cape

Block	Max psm	N, benef.	N, control	Difference in mean	t
1	0,188	8	59	-0,024	-1,645
2	0,399	54	30	-0,003	-0,241
3	0,590	23	35	-0,008	-0,538
4	0,797	32	15	-0,027	-1,547
5	0,965	41	3	-0,019	-0,724
		158	142		

- The estimated propensity scores are split into blocks after size of estimate.
- The largest estimated in each block is given in column 2.
- The column with the header “Difference in mean” given the difference between mean propensity score for beneficiary- and control households in the block.
- The t-values are given from a t-test for difference between score mean for beneficiaries and controls.
- The critical values at a 1% level are given in the last column.
- The propensity scores will be split into smaller blocks if means differ within a block, thus if the absolute value of the t-statistic exceeds the critical value.
- Missing values are due to either that characteristics is not observed in the block, or if there are too few of either control or beneficiary observations in the block.

Table 30: Balance of the covariates in the propensity score regressions for each province

Limpopo

	1	2	3	4	5
Number of plots, pre 1995	-	0,50	0,85	0,40	-
Land all. farm., post1994	-	-	-	-	-
Land all. trib., post 1994	-	-	0,36	-	-
Land all. muni., post 1994	-	-	-	0,80	-
H.h. head male	-	0,42	0,34	0,17	-
Edu. h.h. head	-	0,98	0,72	1,00	-
Age, h.h. head	-	0,90	0,95	0,98	-
Prop. with other farm. exp.	-	0,75	0,12	0,36	-

Mpumalanga

	1	2	3	4	5
Number of plots, pre 1995	-	0,56	0,79	0,53	-
Land all. farm., post1994	-	-	0,47	0,3374	-
Land all. trib., post 1994	-	-	-	-	-
Land all. muni., post 1994	-	-	-	-	-
H.h. head male	-	0,12	0,33	0,41	-
Edu. h.h. head	-	0,51	0,83	0,53	0,12
Age, h.h. head	-	0,24	0,63	0,93	0,41
Prop. with other farm. exp.	-	0,31	0,20	0,77	0,52

North West

	1	2	3	4
Number of plots, pre 1995	0,43	0,48	0,44	0,17
Land all. farm., post1994	-	-	-	-
Land all. trib., post 1994	0,53	0,19	0,34	-
Land all. muni., post 1994	0,53	0,18	-	-
H.h. head male	0,22	0,06	0,89	0,28
Edu. h.h. head	0,55	0,06	0,42	0,92
Age, h.h. head	0,29	0,67	0,82	0,38
Prop. with other farm. exp.	0,20	0,45	0,12	0,22

Gauteng

	1	2	3	4	5	6
Number of plots, pre 1995	-	-	-	-	-	-
Land all. farm., post1994	-	-	-	-	-	-
Land all. trib., post 1994	-	-	-	-	-	-
Land all. muni., post 1994	-	-	-	-	-	-
H.h. head male	0,10	0,37	-	-	-	-
Edu. h.h. head	0,39	0,58	-	0,02	-	-
Age, h.h. head	0,69	0,36	-	0,01	-	-
Prop. with other farm. exp.	0,02	0,59	-	0,26	-	-

Northern Cape

	1	2	3	4	5
Number of plots, pre 1995	-	0,78	1,00	0,92	-
Land all. farm., post1994	-	-	-	-	-
Land all. trib., post 1994	-	0,45	-	0,69	0,58
Land all. muni., post 1994	-	0,65	0,23	-	-
H.h. head male	-	-	0,80	0,74	0,51
Edu. h.h. head	-	0,15	0,20	0,19	0,17
Age, h.h. head	-	0,20	0,96	0,52	0,12
Prop. with other farm. exp.	-	0,54	0,84	0,53	0,15

KwaZulu Natal

	1	2	3	4
Number of plots, pre 1995	0,79	0,16	0,85	-
Land all. farm., post1994	0,19			-
Land all. trib., post 1994	0,62	0,47	0,23	-
Land all. muni., post 1994		0,56	0,63	-
H.h. head male	0,04	0,14	0,75	-
Edu. h.h. head	0,54	0,81	0,33	-
Age, h.h. head	0,81	0,39	0,92	-
Prop. with other farm. exp.	0,59	0,24	0,22	-

Free State

	1	2	3	4	5
Number of plots, pre 1995	0,84	0,79	0,15	0,74	-
Land all. farm., post1994	-	-	-	-	-
Land all. trib., post 1994	-	-	-	-	-
Land all. muni., post 1994	0,34	0,85	0,05	0,37	0,25
H.h. head male	0,09	0,51	0,09	0,24	0,43
Edu. h.h. head	0,51	1,00	0,99	0,60	0,43
Age, h.h. head	0,29	0,33	0,58	0,94	0,45
Prop. with other farm. exp.	0,41	0,92	0,54	0,60	0,77

Western Cape

	1	2	3	4	5
Number of plots, pre 1995	0,19	0,96	0,66	-	-
Land all. farm., post1994	0,67			-	-
Land all. trib., post 1994		0,54	0,92	-	-
Land all. muni., post 1994	0,54	0,83		-	-
H.h. head male	0,94	0,87	0,90	-	-
Edu. h.h. head	0,25	0,19	0,32	-	-
Age, h.h. head	0,60	0,64	0,93	-	-
Prop. with other farm. exp.	0,65	0,75	0,88	-	-

Eastern Cape

	1	2	3	4	5
Number of plots, pre 1995	0,40	0,56	0,17	0,56	0,70
Land all. farm., post1994	-	-	-	-	-
Land all. trib., post 1994	-	0,18	0,82	0,96	0,64
Land all. muni., post 1994	0,40	0,82	0,82	0,50	-
H.h. head male	0,48	0,91	0,98	0,27	0,27
Edu. h.h. head	0,07	0,30	0,52	0,62	0,22
Age, h.h. head	0,08	0,35	0,20	0,98	0,87
Prop. with other farm. exp.	0,06	0,49	0,43	0,42	0,48

- Values are missing when blocks do not contain both beneficiary and control observations, and when differences in means are equal to zero.
- Values are p-values for mean variable value for beneficiaries not being significantly different from the mean value for controls.
- Specification is rejected if any p-values are less than 0.01.

Table 31: Average treatment effect for each province

	N, benef.	N, cont.	ATE	SE	t	t, rejection At the 10% level	At the 5% level	At the 1% level
Limpopo								
Stratification	33	31	113,52	88,68	1,28	1,67	2,00	2,66
Nearest Neighbor	33	12	155,40	89,85	1,73	**1,68**	2,02	2,70
Kernel	33	25	88,76	103,12	0,86	1,67	2,00	2,67
Mpumalanga								
Stratification	62	124	-219,23	118,24	-1,85	**1,65**	1,97	2,60
Nearest Neighbor	63	33	-494,31	310,40	-1,59	1,66	1,99	2,63
Kernel	63	98	-229,36	125,81	-1,82	**1,65**	1,97	2,61
North West								
Stratification	76	102	95,63	116,72	0,82	1,65	1,97	2,60
Nearest Neighbor	76	45	130,39	138,52	0,94	1,66	1,98	2,62
Kernel	76	93	115,01	126,72	0,91	1,65	1,97	2,61
Gauteng								
Stratification	19	55	369,13	149,78	2,47	1,67	**1,99**	2,65
Nearest Neighbor	27	8	432,33	123,95	3,49	1,69	2,03	**2,73**
Kernel	27	26	290,62	294,30	0,99	1,68	2,01	2,68
Northern Cape								
Stratification	27	26	-106,15	503,84	-0,21	1,68	2,01	2,68
Nearest Neighbor	27	9	-92,48	643,09	-0,14	1,69	2,03	2,73
Kernel	27	20	-177,52	660,39	-0,27	1,68	2,01	2,69
KwaZulu Natal								
Stratification	64	354	447,86	180,83	2,48	1,65	**1,97**	2,59
Nearest Neighbor	64	44	385,98	204,29	1,89	**1,66**	1,98	2,62
Kernel	64	339	433,22	185,72	2,33	1,65	**1,97**	2,59
Free State								
Stratification	94	194	86,51	191,87	0,45	1,65	1,97	2,59
Nearest Neighbor	94	62	59,80	200,76	0,30	1,65	1,98	2,61
Kernel	94	189	91,92	190,51	0,48	1,65	1,97	2,59
Western Cape								
Stratification	51	121	11,04	96,30	0,12	1,65	1,97	2,61
Nearest Neighbor	53	35	95,44	101,77	0,94	1,66	1,99	2,63
Kernel	53	102	9,08	80,84	0,11	1,65	1,98	2,61
Eastern Cape								
Stratification	134	166	-615,22	286,53	-2,15	1,65	**1,97**	2,59
Nearest Neighbor	134	55	-556,55	304,68	-1,83	**1,65**	1,97	2,60
Kernel	134	140	-596,28	247,22	-2,41	1,65	1,97	2,59